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a record of research

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HUMAN BIOLOGY

a record of research

DECEMBER, 1930

IS MAN A RATIONAL ANIMAL?

BY WM. E. RITTER

University of California

Most litterateurs of today and a majority of the rank-and-file appear to be ready with an emphatic "No" in answer to the question. They claim that the "New Psychology" (which one does not seem very important) proves this. Moreover they accept it with less hesitation as it corresponds with their personal inclinations. For who does not like to *feel* better than he likes to *think*—think, I mean, in the sense of solving hard problems in geometry or budget-making? But my answer is the opposite of this. Man is a rational animal. Strange as it may seem, this answer I reach primarily as a student of animals generally and only secondarily as a student of human animals. Comparative studies in many parts of the zoological realm lead me to see

[1] Read and discussed at a joint seminar of the departments of psychology and zoology of the University of California, April 3, 1930; and revised in the light of this discussion. The following introductory remarks were made by the speaker:

According to the Platonist-Cartesian-Kantian theory of life biology is the science of the bodies of living beings while psychology is the science of their minds. This theory makes Life a "house divided against itself." What psychology has suffered from a theory of bodiless minds is not for me to say. What biology has suffered from a theory of mindless bodies I could discourse on long and, I think, convincingly. It would be a fine thing, it seems to me, if this joint meeting of psychologists and zoologists might be the first of many similar meetings to the end of replacing the divided house theory of life by a united house theory. This might result finally, for our own University at least, in a philosophy of life symbolized by the fact that nearly all the University departments devoted to living nature are now housed together in this magnificent building, conspicuously labeled *The Life Sciences*.

that in comformity with the general principles of taxonomic biology man must accept being classed as rational whether he likes it or not.

What I want to show is that the theory of man's non-rationality is equivalent to the theory that his brain with its specially developed cerebral cortex is functionally meaningless; that he is not bipedal and erect in any special sense; and that his fore limbs are not particularly free, tactile, and prehensile. Or the logical consequence of the theory may be modified to the extent of saying that although these characters of man are undeniable, they are purely structural and of no special consequence so far as his behavior is concerned. The reasoning that because biology has proved beyond question man's animal nature, therefore man is "nothing but" another animal has gained its great vogue largely, I believe, because biologists have failed to stand pat on some of their best established facts and principles. The facts and principles specially referred to are those involved in description, naming, and classification.

THE IMPORTANCE OF THE COMPARATIVE METHOD

The comparative method has played an important and honorable part in the history of biology. Of late it has lost much of its prestige through the advent of other methods. Notably experimental and quantitative methods have come into great and richly deserved favor. It is however of the utmost importance for solid achievement in any branch of natural science to recognize that no one method will do for investigating all the phenomena of nature and that all these phenomena must be investigated. That is, all phenomena must be investigated if the goal for natural knowledge is that finally it shall become as nearly complete as possible.

No natural scientist has ever contended that it is possible to make any headway in the study of nature without description and classification. And these imply comparison as inevitably as the right hand implies the left.

The importance of the periodic system of the chemical elements and the renewed interest in it awakened by recent discoveries on atomic structure and action well illustrate the fundamentality of classification in this realm of inanimate nature. But all other realms illustrate it quite as well. Rocks and stars have to be classified as well as do chemical elements and grasses.

The great place taxonomy has in biology as compared with what it has in chemistry, astronomy, or any other science of not-living

nature is due chiefly to the fact that the objects of living nature are so much vaster in number, varied in form and action, intimately connected together, and obtrusively present to us humans, that classification plays so much larger a part in our knowledge of the living than of the not-living realm.

An exceedingly unfortunate attitude towards classification has been assumed of late by many biologists from the circumstance that the task of collecting, describing and classifying the plants and animals of the earth has been largely done, in a reconnaissance way. This has permitted the center of interest in research to pass to other subjects. So far this is natural and good. But along with the passage of interest in taxonomy to other subjects there has gone a deplorable tendency to look upon this subject as out of date and relatively unimportant. There has been failure to discriminate between the *work* of description and classification and the *principles* of description and classification. Great emphasis has been placed on, for instance, the analysis and causal explanation of phenomena, these having been largely ignored by earlier taxonomists. Again this is natural and good so far. To the extent, however, that analysis and explanation have been regarded not as supplementing, but as supplanting description and classification, the most basic principles of objective reality and natural knowledge have been violated.

G. K. Chesterton's blistering criticism ("The Inefficiency of Science," *North American Review*, Nov., 1929) appears to hinge on the point here made. His artist's "instinct" seems to have sensed the deplorable fact that so many present-day students of nature are refusing to admit any knowledge to be truly scientific except such as consists of deductions (largely mathematical) from relatively few sense data. In other words, Chesterton seems to suppose that such students have really succeeded in their efforts to exclude from the Temple of Science all those branches of natural knowledge which must, from the necessities of the phenomena dealt with, rely greatly on induction, i.e. on description, definition and classification.

THE MORPHOLOGICAL EVIDENCE OF MAN'S RATIONALITY

What has all this to do with the question of man's rationality? Very much. It brings us to the real point. The question is as to whether we men shall use our heads as *human* animals or merely as *brute* animals use theirs. It is not a question of how much rationality we possess, but of how we use what we do possess. According to the

principles of biological classification, to contend that man is not a rational animal because many individuals fail much of the time to use their apparatus of rationality is equivalent to contending that he is not an upright-postured animal because nearly all individuals lie down or sit down much of the time.

Another form of statement of our question is: Was Linnaeus justified in giving man the species name, *sapiens*, if he intended that name to be really descriptive? The more vehement of the anti-rationalists do not hesitate to pronounce the great systematizer quite wrong in this. We read: "Linnaeus . . . has called mankind . . . *Homo Sapiens*—Wise Man. But this praise is manifestly unjust. For man heaps up such abundant examples of extraordinary folly that, to conform with the reality of things, we should call him the exact opposite: *Homo Stultus*—Stupid Man" (*Idiot Man or The Follies of Mankind* ('L'homme stupide') by Charles Richet).

Generally speaking, those who think it necessary to revise downward man's conception of himself grant that on purely morphological grounds he is entitled to the rather widely removed place in the zoological system taxonomists allow him. Linnaeus, as is well known, recognized man as constituting a single genus, *Homo*, in the family *Hominidae* of the order *Primates*. This makes him as different from the great apes, his nearest animal kin, as a sheep is different from a pig or a horse from an elephant. So even on this basis to dispose of man as "nothing but" another animal is a glaring instance of shoddy thought and talk from the standpoint of sound zoology.

But it is when due attention is given to what man's morphological peculiarities imply concerning the part played by these in his "struggle for existence" that the wretched consequences of such thinking and talking come to view. People who have little or no biological education (the vast majority, more's the pity) cannot reasonably be expected to recognize these logical fallacies and follies. But professional zoologists can hardly escape noticing them and attempting some sort of logical readjustment relative to them. Now those zoologists who share the violent hostility to "anthropomorphism" that came (rather justifiably) into biology with the modern demonstration of man's animal nature, are likely to try to justify the fallacies. How explain man's upright posture and the resultant freedom of his fore-limbs for wide and efficient grasping if he is "nothing but" another animal, since other animals are but slightly possessed of these characteristics? Still more difficult to explain are the most characteristic parts of

man's brain. What under the sun can be the meaning of the chief part of his cerebrum? For it is now known with certainty that this is mainly devoted to processes heretofore called rational. Does it mean that these processes ought to be given some other name? But would naming them something else make them different from what they really are?

Some students of morphological zoology and portions of functional zoology have sensed these logical difficulties and have suggested various ways of escaping them. One suggestion is that the main part of the human brain is not essential to man's real nature. The cerebrum may be a kind of superfluity—perhaps something like the hump on a camel's back or the quirl on a pig's tail. Or possibly it is a sort of non-malignant hereditary tumor or wen. Although suggestions of this sort have been made by serious minded zoologists, they perhaps were made only half-seriously and need not be treated very seriously.

BEHAVIORISM AND THE CEREBRAL CORTEX

Another suggestion which has the merit of being less fantastic is that so little is really known about the function of the cerebral hemispheres that they may as well be ignored in our efforts at a scientific interpretation of our behavior. This suggestion comes with sufficient seriousness to deserve real consideration. The tendency of extreme behavioristic psychology to minimize the importance of the cerebral cortex as a consequence of its proposal to throw consciousness overboard has not attracted anything like the attention it should. Early in John B. Watson's elevation to the leadership of Behaviorism his writings began to show this tendency (in his *Psychology*, for example). The tendency has gone on till in his latest full-rounded doctrinal work (*Behaviorism*, The People's Institute Publishing Co., 1924) the cerebral hemispheres have almost reached the vanishing point and the whole brain figures as a mere part of the spinal cord. In *Behaviorism* (the book) and, by inference, in behaviorism, the school of psychology, Watson takes notice of the accusation that behaviorists do not show enough respect for the central nervous system. One of his statements is this: "For behaviorists the nervous system is, 1st, a part of the body—no more mysterious than muscles and glands: 2nd, a specialized body mechanism that enables its possessor to react more quickly and in a more integrated way with muscles or glands when acted upon by a given stimulus than would be the case if no nervous system were present" (p. 38).

The first part of this statement I endorse entirely and gladly. The second part I endorse with equal whole-heartedness as far as it goes. The proof is conclusive that the nervous system both quickens action and integrates action. But it has another role not mentioned. It "enables its possessor" (note Watson's language) to use its muscles and glands more effectively *to its own welfare* than it otherwise could use them. In other words the nervous system is the animal organism's morphological system that enables it to act adaptively in the highest measure. And the culminating part of this system is the cerebral cortex of *Homo sapiens.*

Consequently if a system of interpreting animal organisms is adopted which minimizes the importance of the central nervous system, it does violence to the best principles of both morphology and physiology and cuts the very heart out of the conception of organic wholeness.

So we look with keen interest to see how the nervous system is treated in *Behaviorism.* We find there a total of eight figures introduced to aid the presentation. Of these one only touches the "central" part of the system. This figure is of a section, diagrammatic to the limit, of the spinal cord introduced to illustrate a point about the reflex arc. As for the cerebral cortex the nearest approach to a mention of it in the text (I hope I have not overlooked any other) is the statement, page 92: "Infants born without cerebral hemispheres exhibit" the same reflexes that normal ones do. It is probably not justifiable to infer from the treatment of the nervous system in this book that orthodox behaviorism would hold the view that men with no cerebral hemispheres would be as well off as those having them, could all the rest of the system be perfect. But in all sobriety one may question whether the treatment justifies the opposite view.

So much in illustration of the queer ways men can use their reason to justify themselves in being unreasonable.

WHAT IS INTELLIGENCE?

Now let us come to the task of comparing the activities of animals generally to discover the difference, if there is any, between the part played by the activities of men and those of all other animals toward solving their life problems.

My field studies on the activities of animals in nature bring me in contact with many people, farmers, foresters, vacationists, and so

on. These are often curious as to what I am doing. When I call their attention to striking instances of adaptive action by many creatures, no question is more common than "Is it intelligence?" As I make no claims as an expert on intelligence, these and similar questions have led me to "read up" somewhat extensively on what experts have written on the subject. Finding to my surprise that almost every expert consulted, especially if he ranks high as an expert, disagrees rather sharply with all other experts, I have been thrown back on my own resources for defining intelligence. Or, to speak more accurately, I have seemed driven to devising some kind of general statement about what I observe animals doing that brings out the question just quoted, but which none of the expert definitions really cover. Why then should I not concoct a definition of intelligence to meet the needs of zoology, since no definition seems to exist that really meets the needs of either psychology or zoology?

Accordingly here you have the results of my efforts: Intelligence is that attribute of which some animal species have become possessed through the long, hard course of evolution, in virtue of which the individuals may avoid doing fool things if they really desire and really try to avoid them.

Perhaps the injection of such a phrase as "fool things" into the discussion will strike you as a bit of facetiousness. Does it seem to mar the proper somberness of the occasion? I hope not. Please recall that the genius of the Platonic philosophy has been characterized (I think truly) as "free and facetious."

The possible charge of facetiousness against my definition troubles me less than the probable charge of ambiguity. But ambiguous it can hardly be to those who must have noticed what men do at times and may have noticed what the lower animals are apt to do. Especially should the questionable phrase be unambiguous for those who think critically on how the things done as hinted affect the wellbeing of the actors themselves and their kind. Furthermore removal of ambiguity may be materially helped by the cooperation of a good sense of humor with critical thinking on the subject. A strong case could be made for the hypothesis that the whole gamut of human action included in the conception of "from the sublime to the ridiculous" sends its roots down to the very bottom of man's nature as an animal organism. Surely all work-and-play or play-and-work can be shown to be rooted that deeply.

"FOOL THINGS" THAT ANIMALS DO

Anyhow it is no meaningless coincidence that the prince of American humorists should have been one of the most deeply-truthful observers of animal activities generally, human and subhuman alike. I am already on record[2] in defence of Mark Twain's competency as both observer and interpreter of the performances of animals. My supplementation of his account of the way ants do things ought to be superfluous in view of the many other confirmations, by professionals, of his testimony. But somehow the facts do not seem to impress experts on intelligence. The funny side of the performances almost anybody can see but their serious side appears to escape almost everybody. That this side did not wholly escape the man, S. L. Clemens, is pretty certain from indirect evidence furnished by his life and writings.

Such a mass of "fool things" done by animals is presented in the book just referred to that it seems unlikely that just more would strengthen the evidence. Accordingly the few additional facts I give here I give with the hope that their setting and mode of using may bring to light more of what I take to be their deeper meaning.

Mark Twain's "Baker's Blue-jay Yarn" in *A Tramp Abroad* I hold to be transformed from good story telling into good zoology by my studies and publications on the operations of the California woodpecker.[3] In the Quarterly Review article I tell, as one example of "fool things" done by the birds, about their making many little holes through the siding shakes of a cottage, into which holes they put acorns; but as the holes open into a deep space behind the shakes

[2] *The Natural History of Our Conduct*, p. 119, Wm. E. Ritter with the collaboration of Edna W. Bailey.

[3] Besides what I have said about these birds in the book above referred to and earlier publications, the following later publications may be noted: "An untilled field for a revised kind of research in zoology," *The Condor*, Vol. XXXI, pp. 160–166, July, 1929. "The nutritial activities of the California woodpecker" (Balanosphyra formicivora), *The Quarterly Review of Biology*, Vol. IV, No. 4, pp. 455–483 1929.

"Tom Sawyer" and "Huckleberry Finn" have given Mark Twain considerable distinction as a human psychologist. I submit that any one who will ponder certain of his jokeless writings along side various of the animal yarns, notably "Eccentricity of the Ant," must allow that not merely Mark Twain but C. L. Clemens deserves serious attention as a psychobiologist. The ideas of "Self," of man's "Interior Monarch," of "Outside Influence," and of "Training" set forth in the hundred-page dialogue, "What is Man?" come nearer the central truth of human nature than one sometimes finds in a thousand-page professional treatise on philosophy or pedagogy.

the inserted nuts drop into this and are a dead loss to the birds. A recent visit to this cottage happened to be on the day that workmen were stripping off the much be-punctured shakes preparatory to mending the damage done by the birds. The quantities of old, insect-destroyed or otherwise meatless acorns that were rattling to the ground as the shakes were torn away was a ludicrously impressive sight, sure enough. There were certainly thousands of them. After scooping up one market bag full for my own collections and another for Professor J. Arthur Thomson, who was my honored guest on this occasion, and after selecting samples for us both of the old, riddled shakes, we turned away eagerly questioning how such things can be. How comes it that the very same creatures can perform acts that look so much like intelligence as do many of the storing activities of these woodpeckers, but also acts which look so absurd as does this particular performance? The acorn crop in this locality being abundant, holes in red-wood shakes being easily made, and the birds having plenty of time one may, perhaps, question the justification of calling the action "foolish," "absurd" or by any other derogatory term. It may be legitimate as play or something else the significance of which we do not recognize. But anyway quantities of acorns are stored, nominally for food but practically where they can serve no such end. As to its main purpose this particular performance is certainly futile. Since, however, there is little or no evidence that the birds suffer inconvenience from the loss of the acorns, whether or not one regards the performance as ridiculous—laughable—would seem to depend on what sort of things excite his risibles.

SELF DESTRUCTIVE ACTIVITIES OF ANIMALS

But now let us notice another activity by these very same birds. In my Quarterly Review article I tell of the killing by a neighboring orchardist one summer of a large part of the whole settlement. This was done because the birds were playing havoc with his crop of almonds. Most of the birds were shot in the very act of taking the fruit, as I can testify from having seen many decaying carcasses under the trees.

The question of whether the orchardist was justified in thus breaking the bird protection law to save his crop is not here raised. Rather the question is, whether the birds were justified in preying on the crop. Were they under such straits for food that it was almonds or starvation? Not at all. At the very time they were committing

depredations on the almonds there were quantities of unused acorns they themselves had stored in oak trees only a few hundred yards away. Nor were these acorns inaccessible as were those lost in the cottage.

The bald fact is that large numbers of the birds paid the death penalty for feasting on almonds when by eating acorns of their own providing they would have been free from such a fate. Quite likely young almonds, full-grown but not yet hard and dry, were a welcome change from the usual diet of old dry acorns. But was the difference enough to justify the risk of death? It is so obvious that birds do not act as though they think about their welfare in this fashion, that it seems to many persons unnecessary or even unfair to raise such questions. But why not raise such questions? Indeed how avoid raising them if we would be consistent? The very concepts of thought, of reason, of intelligence appertain basically to the human animal alone. They were originated by men for application to men. Consequently if we see occasion for applying them to other creatures at all, how can we consistently neglect to go all the way in the application? If we would pronounce men to be acting with poor judgment, bad reasoning, small intelligence, "foolishly," who should forfeit their lives under similar conditions, how avoid the same verdict for the woodpeckers? It is no less true for science than for common experience that "it is a poor rule that will not work both ways." The question of whether we shall or shall not be "anthropomorphic" is not raised. What is insisted on is that if we choose to be anthropomorphic at all we shall be so consistently. If we call acts of brute animals intelligent because they resemble acts of human animals which we call by that name, then we are logically bound to apply to the acts of brute animals whetever terms we apply to the acts of human animals which we regard as not intelligent.

In such cases as just narrated, badly directed action is not merely foolish; it is tragic. And a great point is that it is not exceptional. As to type it is common among all classes of animals. Accept in their fullness the principles of biological classification, and apply the criteria of the wisest and best human action in a systematic study of natural activities in the whole animal world as affecting the well-being of the acting creatures themselves and their kindred, and you cannot help seeing innumerable performances which grade from useful or harmless play all the way to laborious work that may be extremely wasteful and destructive even to the lives of the actors.

EXCESS IN ACTIVITY

The well-nigh ceaseless running and jumping; falling, rolling, and tumbling; and the amusing pranks of the young of practically all domestic mammals and of healthy children are as good illustrations as one needs of the playful aspect of activity. But even here the constant tendency to excess, often highly injurious, in the play of children, is especially to be noted. For of all the varieties of "foolishness" to which animal activity tends, probably no other has had a more important part in the kinetic aspect of evolution than excessiveness.

This general tendency to excessive action shows itself most glaringly perhaps in reproduction. At least the "geometric ratio of increase" of organisms, utilized by Mr. Darwin as one of the corner stones of his theory of natural selection, has given this aspect of the tendency its greatest theoretical prominence.

As to activity in the more usual sense the tendency to excess shows itself nowhere perhaps more strikingly than in connection with food, especially in such animals as have become collectors, transporters and storers of their food materials. Although a systematic investigation of the subject seems never to have been made, apparently all species that do these things are ever liable to do "fool things." This liability is not more striking in any direction than that of going further than is necessary to accomplish the original purpose of the action in case the objects toward which the action is directed happen to be abundant. No animal below man seems to have any other inhibitory power over its actions than satiety and fatigue.

All of us animals, brute and human alike, are in the very nature of our ability to act at all, constantly liable to do the wrong things and overdo the right things. If I am an earthworm my responses to moisture, light, temperature and other stimuli expose me to a variety of dangers, including that of being run over and crushed to death by creatures much larger, heavier and more active than I am (*Natural History of Our Conduct*, p. 184). If I am a harvester ant and depend on collecting and carrying home seeds of various plants, I am everlastingly liable to collect objects which, though somewhat resembling seeds, have not the slightest food value. Furthermore often when successful in getting useful seeds I am boggled into carrying them miles where inches were entirely sufficient (*Nat. Hist.*, p. 120). Nor am I by any means free from liability to foolish, even tragically foolish, acts when I have evolved to the dignity of a vertebrate. For as

fish, salamander, or frog, some of my performances in feeding and in mating reach the climax of bestiality, understanding this term at its worst (*Nat. Hist.*, pp. 165–169).

The varieties of foolishness to which my actions are liable when on the whole they have evolved to the grade of wisdom represented by the California woodpecker, I have observed in great particularity. The usefulness of storage-hole pecking carried to uselessness by pecking many holes in which nothing is ever stored or pecking new holes when plenty of perfectly good old ones are available; the storing of acorns entirely beyond the reach of the storer; the storing of pebbles and other useless objects—these are only illustrations of a general principle.

The mammalian state having been attained, it would seem that doing fool things should be out-grown, evolved beyond. But not so. If I am a beaver, a creature commonly held to be industrially classifiable as an engineer, I am still liable to carry my usually wise activities to foolishness quite as arrant as while I am an ant or mud-dauber wasp or woodpecker or woodrat. For what could be more ludicrously futile than performing the acts of dam-building in a pool of still water or of gnawing down an old dry tree to get green bark and twigs for food? (Unpublished notes of mine on beaver work contain observations of both these things.) But beavers may surpass woodpeckers in the tragic as well as in the harmlessly foolish character of their performances. For proportionate to their efficiency in dam building and tree cutting is their liability to push these activities to serious self-injury. Where beavers are near neighbors to men it is apparently usual for them to become so destructive of men's belongings with no essential advantage to the animals as to bring upon themselves the full force of man's ability to inflict the death penalty on anybody that he regards as seriously injurious to him (*N. H. C.*, pp. 156–158 and 220).

WHY IS MAN THE DOMINANT SPECIES?

This last statement is illustrative of a principle of great importance for this discussion. The principle may be indicated by asking the question, How comes it that the animal species, *Homo sapiens*, has been and is so destructive to all other species with which he comes into competition? Man's "commercial greed"; his inordinately developed "hunting instinct"; his "heartlessness" and disregard for the "rights" of the inferior creatures; his possession of fire-arms and deadly contrivances of various kinds—such are the familiar answers to the question.

Much of truth as there is in these answers, from the standpoint of a rigorous taxonomic zoology, they miss the main point. They leave untouched the question of what there is in the morphological difference between the human and other species that gives the human species the competitive dominance implied by the answers? To this only one answer is possible: man's nervous system, his cerebral hemispheres especially, and the chief structural attributes coordinated with this, namely his erect posture with his consequent free and wonderfully efficient fore-limbs, tell this part of the story fully.

But, as previously indicated, this, the structural side of the story, is simplicity itself as compared with the other, the activational side.

When will the Delphic oracle's injunction "Know thyself" have been so far obeyed by man that he shall have become the beneficiary in the fullest possible measure of his attributes as just noticed!

The query "Is it intelligence?" previously noted as a common response to my demonstrations of the strikingly adaptive activities of woodpeckers and other creatures has a counterpart quite as common and quite as significant. When I go on and demonstrate the "fool things" which the same creatures do, the response is fully as ready, "Why, human beings act just that way." This response is particularly apt to come when such overdoing of things as food-storing animals frequently exhibit, is pointed out, and as is exemplified by the common remark about birds and mammals which become pests of farm crops, that they "destroy more than they eat."

Replying to this I try to make the point that all human beings act much that way *part* of the time, that some act much that way *most* of the time, but that a few *rarely* act that way. This along with other facts, proves, I go on to say, that the human animal is really very different from any sub-human animal in that it is *possible* for him to avoid "acting that way." But my efforts have never succeeded very well in making this point. I am, consequently, going to try again, now, though very briefly.

The lower animals do "fool things" because they can't help it. They have no mechanism for avoiding it. Said in twenty-three words we have: *No animal below man has a cerebral cortex or any similar structure by the use of which he can avoid doing fool things.* His entire mechanism is such as to enable him, in his own specific way, to do "wise things" as a rule (i.e. act in accordance with the welfare of himself or of his kindred, one or both). But if "the rule" of his acting changes a bit, since he has little or no equipment for changing his

action correspondingly, his normally wise acts become foolish, even tragic. It is first and foremost a problem of individual equipment for guiding individual action in behalf of individual welfare. But the individual is always a member of a species, or kind. He can neither come into existence nor reproduce himself under any other conditions.

So it is that with the species, *Homo sapiens*, the problem of taxonomic zoology merges into and fuses with the ancient human problem of choice and freedom of action. Zoologically viewed it is beyond question that the human animal far surpasses all other animals in its equipment for controlling its actions to its own good. The most definitive of man's morphological attributes as above noticed makes doubt on this aspect of the matter impossible.

But since in its very nature such control involves choice—selection—as between alternative possibilities of action in particular instances, the real problem is as to how far man lives by his most defining attributes.

THE TWO ASPECTS OF INTELLIGENCE

It turns out then that my definition of intelligence is only the negative side of what is really aimed at. "Fool things" are such only as the obverse of "wise things." And "wise" appears to be the word most in favor with moralists of the Anglo-Saxon speech for designating things which, psychozoologically considered, are in a high degree adaptive; that is, are truly advantageous to the acting individuals or their kind, one or both. Justification for this negative way of defining intelligence lies in the enormous difficulty there is in deciding what *not* to do in many of the most crucial situations of human life.

Linguistically my definition seems to differ mainly from current definitions in the meaning attached to the basic part of the ancestral word, *intellectus*. According to the lexicons the primary meaning of the Latin verb *legere* is *to gather, to collect, to put together*. But an accessory meaning is *to choose, to select, to pick out*, and it is clear from the illustrations that the choosing referred to was on the basis primarily of what was supposedly good for man.

The prevailing efforts at defining intelligence seem to rest on the primary meaning of the original word. Mine on the other hand rests more on its accessory meaning. So far as I can see, the measurement of intelligence (which now seems to dominate interest in the subject with many psychologists) relates mainly or wholly to learning, that is to gathering or collecting, data. What use the learner makes of

the data seems not to figure much in the problem as these students treat it. Choice by the learner as to what shall be learned on the basis of the learner's own welfare seems to enter very little into the prevailing conception of learning. I do not find much serious attention given to the extent to which, as psychologically viewed, each individual's welfare is in his own hands. Self-responsibility in a truly vital sense is apparently assumed to be largely replaceable by parental, school, governmental, social and other forms of extraneous responsibility.

The fallacies that lurk in this assumption are too varied and subtle to be dealt with here. But they are largely amenable to analysis.

THE FREEDOM OF THE WILL

There is, however, a great realm of human experience into which the psychozoological problem merges and which is so unescapable and ever-present as to have been one of man's major problems throughout his entire cultural history. The problem referred to is none other than that familiarly known as the freedom of the will, or simply of Freedom. The whole vast round of man's activities on which his highest welfare depends and which has become more or less subject to personal control in each particular instance, we of the Anglo-Saxon world have generalized and named the Will. That is to say, essentially this must be the meaning of the word according to the standpoint of this essay, if it has any real meaning at all.

That man has a wide range of choice as to how he shall act in all the typical situations of life is as certain as are his definitive morphological characteristics by means of which he is able to act at all.

Man's real question is not as to whether his will is free, but as to how he uses the equipment for freedom he certainly possesses.

It looks as though this idea of "fool things" has about the same meaning that the ideas of "sin" and "wickedness" had in former days when men used these terms.

SOPHROSYNE

The ancient Greeks appear to have been the first to make a real start toward recognizing the great possibilities for man's good there are in his ability to regulate his actions and impulses by means of what we now know to be his cerebral hemispheres and their fundamentally related corporeal parts. Greek scholars and artists laid great store on this ability and had a name for it that has never become current in our language and seems to have no exact equivalent with us. The

name is *sophrosyne*. *Moderation* is the English word that corresponds to an important part of the meaning of the Greek word. But neither that nor any other single word of ours covers the whole of its meaning. "We may translate it," writes a highly competent student of Greek philosophy, "sobriety, moderation, discretion, temperance, sagacity, wisdom, self-mastery, modesty, chastity." (F. J. E. Woodbridge, *The Son of Apollo*, 158) "The thing it stood for," Woodbridge goes on to say, "was prized, for he who possessed that thing could be sure, and give others the assurance, that his mind was sound and himself safe except for the tricks of fortune."

If any one questions whether it is really worth while for men to try to regulate their acts by the distinctively human part of their brains, instead of letting them run on with little regulation beyond such as appertains to the woodpecker part of their brains, he may get light on the question by consulting some of the millions in this country who are today paying the penalty of over-activity or ill considered activity in the stock market, or in various of the industries, agriculture and the oil business, for instance, where overproduction is reaping, or is likely soon to reap, the harvest of its own sowing.

Much of modern life, particularly in business, and in sex affairs, appears to be based on the theory that the Greek idea of *sophrosyne* was a delusion and that man's cerebrum is a huge by-play of organic evolution.

We may now conclude with a summed-up answer to our initial question: "Is man a rational animal?" If the answer is given in the motif and temper implied by the form of the question, that answer must be "Yes, man is a rational animal." This must be the answer because man is so vastly more rational than is any other animal.

The meaning of both question and answer is contingent on the meaning of the word "rational."

According to the facts and conclusions we have sketched, if the term means anything at all it means in final analysis the ability of an animal organism to consciously guide its actions in accordance with the wellbeing of itself and its own kind.

THE INTELLIGENCE QUOTIENT AND THE PHYSICAL QUOTIENT: THEIR FLUCTUATION AND INTERCORRELATION

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I. RELATION BETWEEN PHYSICAL AND MENTAL DEVELOPMENT

A. Statement of the Problem

The problem of the relation between physical and mental development has an interesting history. The studies go back at least to Quetelet, who from 1835 to 1871 emphasized the importance of measurement of human qualities. His "Anthropometrie," published in the latter year, made a profound and wide spread impression. Centuries before the publication of Quetelet's "Anthropometrie" artists, like Leonardo da Vinci, had been concerned with the human proportions. Of such artists Quetelet (1871, pp. 166, 167) gives a table. During our Civil War, 1863, studies of the physical dimensions of American soldiers were considered and Joseph Henry, secretary of the Smithsonian Institution, "caused apparatus to be constructed for the purpose" at the Coast Survey Office. A little was done that year; and still more later under the competent direction of the astronomer, B. A. Gould. The results were published, 1869. No doubt B. A. Gould, Henry P. Bowditch and Francis Galton were influenced by Quetelet's book to make human measurements. It was undoubtedly Bowditch's work on the growth of Boston school children (published 1877), that led G. G. Tarbell to weigh and measure all of the children in his institution at Boston for feeble-minded children. Tarbell read his paper at the Frankfort, Ky., meeting of an organization of officers of institutions for the feeble-minded in 1881. The paper was published in 1883. He compared his data with those of Bowditch

as a standard and published a diagram of comparative growth curves. He reached results that are here worth reproducing, since the original is not easily obtainable.

> First, that idiotic and feeble-minded children in our school throughout their period of growth are about two inches shorter and nine pounds lighter than normal children of the same age.
>
> Second, that the relative rate of growth of the two sexes of idiot children corresponds very nearly to that of the two sexes of normal children, and is subject to the same variations at the age of puberty;
>
> Third, that the period of puberty is about two years later in idiots than in normal children.

Under the second result Tarbell includes the fact that the adolescent spurt in growth comes earlier in girls than in boys.

Since Tarbell's paper was published and distributed in 1883 it was probably responsible for the fact that G. E. Shuttleworth, of Lancaster, England, gathered together heights and weights of 1209 idiots and imbeciles in Earlewood, Royal Albert and Lambert Asylums, and compared them with the heights and weights of the general population, as furnished him by Charles Roberts, the leading English anthropometrist of his day. As if in recognition of his debt to Tarbell, Shuttleworth sent his paper to the above mentioned Association of American Medical Officers and it was read by title at their Glenwood, Iowa, meeting in October, 1884, and later published in the Proceedings in 1886. Shuttleworth's table shows clearly that idiots are, on the average, shorter and lighter than the general population.

In 1891 W. T. Porter, professor of physiology at the St. Louis Medical College, began his measurement of St. Louis children, following in the footsteps of Bowditch, the physiologist of Harvard. As a by-product of this work was Porter's paper of 1893 in which he showed that precocious children are heavier, taller and have larger chest girths and wider heads than dull children. Porter (1893, p. 174) later discovered that two Russian observers, Gratsianoff (1889) and Sack (1892) had reached a similar conclusion. Gratsianoff found that successful pupils are larger and grow faster than unsuccessful ones and Sack, who measured 4245 boys (2600 of them twice) found that the more successful children were taller and had larger chests than the unsuccessful. In the Chicago Schools Christopher (1900), following Porter's method, found that of 12-year-old children scattered through the various grades, from I to VIII, those of the Ist grade averaged as low as 1300 mm. stature while those of the VIIIth grade had an average

stature of above 1450, and these results were elaborated for various dimensions by Smedley (1900) on the same children.

These results of Porter and Smedley did not go unchallenged. Gilbert (1894) made a series of accurate determinations of sense reaction, of suggestion, of reaction time and fatigue, and other capacities of school children, aged 6 to 17 years. He considered, also, weight and height of the children. Apparently using the judgment of teachers as a basis of mental ability, he states that he can find no relation between height and mental ability. But it does not appear that he investigated this matter critically enough to warrant a definite conclusion. Gilbert again, in 1897, expressed the opinion that there is no relation between weight or height, on the one hand, and mental ability, on the other. He stated, indeed, that at ages 10 to 14 the dull children are much heavier; but he apparently was expressing a general impression merely. West (1898) found the "poor" students at Toronto "more fully developed" (taller) than "good" ones. Wissler (1901) measured 60 to 70 Columbia students in successive years from the freshman year onward. Using a dynamometer he found no correlation between class standing (success in class work) and grip; r is -0.08. But Carman (1899), Smedley (1900) and Schuyten (1902), using a wider range of ages and a less selected group, found a positive correlation between strength of grip and intelligence and this result has been repeatedly confirmed.

From these beginnings data rapidly became available on the relation of physical development to mental development. No attempt will be made to cover the whole literature, since this work was well done by Doll (1916) up to the date of his paper. Attention may, however, be directed to the excellent papers of A. R. T. Wylie (1899 and 1903) which discovered the greater variability in physical dimensions of defective as compared with normal children.

Wylie (1899, p. 53) concludes from measurements of height and weight of 161 feeble-minded boys and 174 girls that "feeble-minded children are subnormal in height and weight". He noted also that "Development is delayed among the feeble-minded." They continue growing later than normal children. Also he concludes that feeble-minded girls depart less from the normal than feeble-minded boys.

Wylie (1903) returned to this subject some years later; after having measured more feeble-minded children, he reached the same conclusion as before: "The feeble-minded are subnormal in height and weight. The average of the feeble-minded approximates the minimum of the

normal and the maximum of the feeble-minded the average of the normal".

Norsworthy (1906) and Mead (1914, 1916) emphasized the correlation between degree of mental defect and of inferiority in physical development. Goddard (1912) gathered together data on 11,000 feeble-minded children and these he divided into three groups; idiots, imbeciles and morons. He showed that the idiot, imbecile and moron form a series in successive members of which height and weight in both sexes, at each age, increase. He draws these weighty conclusions:

> The low grade (idiot) has not only a disturbed brain formation but his entire organism is disarranged and growth processes upset.
>
> In the imbecile, the same is true but to a less extent. In the moron we have the interesting phenomenon of a practically normal growth during the immature years, but an arrest of growth earlier than in normals.
>
> Sex differences are less and less marked as we go down the grades of defect.

Goddard's figures show that the curves of male and female heights decussate in the moron at 12, and again at 13½ years; in the imbecile, at 11¾ and 13 years; and in the idiot not at all. Also in weight the decussation of the sex curves takes place in morons at 9 and 15 years; in imbeciles at 11 and 14¾ years and in idiots at 11 and 15½ years. Thus, in idiots, the female sex spurt is lacking in height but not in weight.

Whipple (1914, p. 71) gives so thoughtful an interpretation of the relation we are considering that it seems worth while to quote him in extenso.

> The apparent correlation between height and mental ability raises an important question which reappears whenever we discuss the correlation between any physical trait, e.g. weight, strength, vital capacity, etc., and mental ability. The trend of evidence is to the effect that all such correlations, where found, are largely explicable as phenomena of growth, i.e., as correlations with relative maturity. (Cf. Boas, *Sci.* 3, 1895, p. 225, and Wissler.) This makes intelligible the fact that, in general, the positiveness of all such correlations lessens with age, and that many of them, indeed, become difficult or impossible of demonstration in adults. Thus, to take the correlation in question, a positive correlation is not, of course, to be interpreted as meaning that, taken individually, all tall boys are, ipso facto, bright boys, but that, taken collectively, those boys whose physical condition is good, whose growth is unimpaired by ill health, faulty nutrition, etc., and who realize to the full the possibility of physical development inherent in them (whether they be ultimately short or tall) will be found to exhibit the best mental condition and the most rapid mental development.

Further support for Porter's findings was derived from the results of Arnold (1916), who weighed many children in the New York City Schools and concluded that the heavier the child for his age the higher his school grade.

Doll (1916) made careful measurements of stature, sitting height, weight, grip and vital capacity of a fair number (5 to 41) of feeble-minded boys and girls of various mental ages, 1 to 10 years. In his discussion, Doll considers always measurements in relation to Smedley's norms, a not very fortunate proceeding in view of the heterogeneous racial constitution both of Chicago school children and of Vineland inmates. However, it is sufficiently plain that there is a high correlation (0.30) between mental age and stature. The morons approximate the normal. Doll concludes that "the feeble-minded not only grow at a retarded rate but also cease growing at an earlier age". This "negatives for the feeble-minded at least, the theory affirmed by some writers that children who grow at a retarded rate continue their growth to a later age". Doll finds (pp. 52–54) that the feeble-minded are below normal in sitting height (which is correlated closely with stature). He finds (p. 54) that "the averages in weight for [feebleminded] boys are below normal at all mental ages but 9; those for girls are above normal after mental age 5". "The higher types exceed the normal average, slightly for boys but markedly for girls". Thus weight is closer to normal than either standing, sitting height or grip. In the case of right hand grip Doll (p. 55) finds a correlation with mental age of nearly .70. Left grip is even more highly correlated.

The total average percentile of stature, sitting height, weight, the right and the left hand grips and vital capacity shows a correlation with mental age of approximately 0.60. It is to be noted that the percentile is a ratio and it is possible that a spurious correlation (of an unknown degree) with mental age is introduced.

A step in advance was made by Naccarati (1921), who set out to compare with intellectual development not one physical dimension, like height and weight, but a morphological index which should give a picture of the entire physical development. His subjects were college students. His morphologic index is the ratio of length of limb to volume of the trunk. "Length of limb" is obtained by adding together the length of one arm and one leg. Length of arm is defined as the distance from the margin of the acromion process to the styloid process of the radius. Length of leg is defined as the distance

from "the upper margin of the pubic bone to the exterior malleolus of the foot". "Volume of trunk" is obtained apparently by the method of De Giovanni (1905) and Viola (1905), namely, by adding together three indices: (*a*) the thoracic, or the product of length of sternum × transverse thoracic diameter × antero-posterior thoracic diameter; (*b*) the upper abdominal, or the product of xipho-epigastric length (the level of the lower margin of the tenth rib) × transverse epigastric diameter × antero-posterior epigastric diameter; (*c*) the lower abdominal, or the product of pubo-epigastric length × transverse pelvic diameter × antero-posterior epigastric diameter. Although the epigastric dimensions must lack precision, from the insufficiency of land-marks, Naccarati found in 75 subjects a correlation between intelligence (as measured by Army Alpha, or Otis or Thorndike intelligence test) and morphologic index of 0.356; between volume of trunk and intelligence of 0.36, the probable error in both cases being near 0.06. Others (e.g., Gates, '24 and Sheldon, '27) using Naccarati's method have found a much smaller positive correlation; and Heidbreder ('26) no correlation at all. Garrett and Kellogg (1928) computing morphologic index from photographs of Columbia freshmen got very small correlation.

Baldwin (1922) offered rather more precise correlations. He found in 49 school girls correlations between height, weight or total area of carpal bones, on the one hand, and mental age, as determined by the Revision of the Binet scale, as follows:—

Height and mental age	0.89
Weight and mental age	0.71
Area of carpal bones and mental age	0.83

These are vastly higher correlations than any one else has got. Apparently the result is obtained by using repeated measurements of the same children; not taken, consequently, at one age. Under such circumstances the correlation is largely spurious. Baldwin sees this difficulty and gives correlations for the 49 girls, *age constant*, as follows: height and mental age, 0.53. That is, he concludes, physiologically accelerated girls (and the same holds true for boys) are mentally accelerated.

Severson (1922) testing 100 ten year old school children of Minneapolis found in various grades a correlation between "Stanford Revision" age and roentgenographs of carpal development of 0.38 ± 0.06. The correlation between IQ and carpal age was $0.31 \pm .06$. Other correlations are as follows:

Mental age and height	0.21 ± 0.06
" " " weight	0.15 ± 0.07
" " " head girth	0.17 ± 0.07

In general the correlation of physical dimensions with IQ was less than with mental age. Carter and Freeman (1924), with high grade private school children found the correlation between carpal age and mental age (chronological age constant) to be practically zero.

Murdock and Sullivan (1923) experimented with about 600 pupils of European descent found in the Punakou school, Honolulu, including all grades from first to twelfth. The intelligence quotient was found for each pupil using Otis Primary National Intelligence, or Terman Group tests. Certain corrections were applied to these indices to fit the conditions of the Honolulu school. Physical dimensions used were: stature, body weight (with indoor clothes), average of head length and head width. Instead of using absolute stature, weight and head diameter, deviations from average of each sex and age group were used. Intelligence quotients and deviations from average were used in order to make possible the direct comparison of all of the subjects in one group. The results showed correlations: between weight and intelligence in boys of $0.16 \pm .04$, girls $.13 \pm .04$; of head size, boys, $.20 \pm .04$, girls, $.27 \pm .03$. Separating out the preadolescents alone gave $r = .21 \pm .05$ between stature and IQ, the highest coefficient obtained.

Lutz (1924) confirms the positive correlation between height-weight indices and intellectual supremacy in school children and finds also that pubescence begins about 2 years earlier in the superior group than in the dull group.

Johnson (1925), using as subjects children of the "City and Country School" (above 3 years in age) affiliated with the Bureau of Educational Experiments and also the Nursery School of the Bureau (under 3 years of age), finds for children of any one age no tendency for mental age to increase with increasing weight at given heights. The correlation is only 0.04 ± 0.05. Abernethy (1925) reached about the same result with 359 girls of the University of Chicago Laboratory schools.

Heaton (1925) experimented with 102 boys and 93 girls, ages 11 to 13 in a public school. In any one year of age the numbers were small, 11 to 24. The children had been previously given one or more group intelligence tests. The high IQ group came from the very top of the list; the low IQ group from the very bottom. For physical ratings were used height, weight, 75 yard dash, chest expansion, "push up"

from floor and running broad jump. The differences in output of the two groups was greater (though in same sense) in the case of boys than of girls. Considering the boys only, the high group was slightly superior in height, markedly so in weight, and clearly so in each of the performance groups. The observer gets the impression that the low mental percentile children try hard but are not as effective as the high mental percentile children. It is obvious that just the physical under-development of the low percentile children did not enable them to expand the chest, or jump as far or "push up" as many times. Comparison of children on chronological age handicaps the low percentile children, who are *retarded* in development both physically and mentally.

Heidbreder (1926) with subjects taken from entering university students finds that height ÷ weight ratio and percentile mental scores are correlated as -0.01 ± 0.03, which means there is no correlation.

Cozens (1927) correlated various physical tests (including total scores from the 100 yard dash, 13 foot rope climb, running broad jump, running high jump, 12 pound shot put and discus throw) with the Army Alpha test, in 337 university men and got a correlation coefficient of $-.0018 \pm .0367$, or no correlation. Of course in this case we are comparing not so much constitutional capacity in use of the muscles with intelligence as training in their use; and just the brightest of young college students are a group whose muscular training has been less attended to than that of the less bright. Nevertheless, the brightest students do not fall behind the less bright in "big muscle" tests. This result accords with that obtained by Bagley (1901) and Landis, Burtt and Nichols (1923), who found no correlation between motor ability and mental output in school children.

Cattell (1928), making use of measurements made on normal and feeble-minded children, correlated mental age, as determined by the Dearborn and Otis intelligence test, with total number of permanent teeth for various ages for boys and girls separately. The coefficients of correlation lie close to $0.11 \pm .07$. However they are uniformly positive for the various ages in both sexes. Perkins (1926) testing 555 maladjusted Chicago school children had previously found a correlation coefficient between intelligence and number of erupted teeth of $+.47 \pm .01$.

Cattell (p. 84) also gives the coefficient of correlation between mental age and "anthropometric development". The latter includes

height, weight and iliac diameter combined as follows: "the amount that each child deviated from his age-race-sex norm in each of the three measurements was found in terms of quartile deviations, and the average of the three deviations was taken as a measurement of anthropometric development. Thus a child who deviated from his group norm −0.5 of one quartile division in height, +1.0 in weight, and +1.0 in iliac diameter would have an anthropometric development of +0.5." "This method", she states "will not hold except where all of the children are of the same age. Hence, in using 500 children of mixed ages the average rank in each of the three dimensions is taken as a measure of anthropometric development". The coefficient of correlation between mental age and these anthropometric data varies in the different ages from −.03 to 0.31 with probable errors of .06 to .09.

Cattell finds also the correlation between mental age and anatomic index. The latter is found by dividing the sum of the diameters of all of the carpal bones of one hand by the diameter of the wrist. The correlation-coefficients vary from −0.29 to 0.24; probable errors 0.06 to 0.09.

Using the method of multiple correlation Cattell (p. 86) finds a value of r ranging from .40 to .50. She concludes that, were it possible to correlate the mental age with a (still uninvented) index of physical maturity, a considerably higher coefficient would appear; perhaps as high as .60.

Finally may be mentioned the work of Wheeler (1929), like Cattell's done at Dearborn's clinic on North European children from the public schools of Cambridge, Mass. In all physical measurements (except trunk length and iliac width in girls) dull children are definitely below the normal. This result can be partly accounted for by the other fact that dull children are retarded in developmental stage and mature late in chronological life.

To sum up, a correlation between physical and mental development in children surely exists. It is most obvious when the two series are both long and show marked differences at the extremes. Thus a correlation will be found in a series comprising both feeble-minded and superior children, when it may not be found in so highly selected and uniform a group as a lot of college students. The failures to find a correlation have mostly occurred in reporting on a too-homogeneous or a non-developing group.

Also the criterion of physical development must be adequate. Just stature alone, or weight alone, although probably the best single

criterion of general development, fail of giving as complete a picture of physical development as the sum of various physical events such as tooth development, body hair development, the development of the carpal bones and the changes in bodily proportions.

It was the purpose of the present study to compare the physical development based on several criteria with the mental development of children having a large range of IQ's.

B. Methods and Material

In December, 1923, Miss Minogue drew up a list of 100 boys at Letchworth Village on which mental and physical measurements were to be taken annually and on each boy at about the same time. Through discharge, transfer, illness and death this number was much reduced. Later 19 boys were added to the list. In the study are included 78 white boys on whom physical and mental measurements were made two or more times—usually five or six times. They are probably all born in the United States. An attempt has been made to estimate the European (or other) stocks involved. The results are given in Table 1.

TABLE 1

Showing "racial stock" composition of the 78 boys forming a basis of this study

U. S. and U. S.-English	39	Czech, Slav	3
English-German	1	Italian	11
German	3	German Jew	1
Irish	5	Roumanian Jew	2
German-Irish	1	Russian Jew	2
Scandinavian	1	"Jew"	3
Dutch-Bohemian	1	Unknown	3
Polish	2	Total	78

Of the boys measured the youngest was 5.29 years at his first measurement. Eight or nine years was a common age of beginning. The oldest boy was 19.70 years at his last physical measurement. The measurements usually ended at around 13 to 15 years, at the time when mental maturity is supposed to be achieved.

From an examination of the boys listed, we sought to find an answer to our main inquiry; How much of a correlation is there between physical development and mental development?

The solution of the problem requires, first of all, a method of measuring mental or intellectual development. It requires, second, a satisfactory method of measuring physical development.

Mental development is fairly well measured, it is generally conceded, by the standard intelligence tests. In the present study the Stanford (Terman) revision of the Binet-Simon test was employed. The test was applied mostly by one person, namely Miss Blanche Minogue, Psychologist at Letchworth Village, but in part by others trained by her at that Institution. Thus the mental age of the subjects was secured.

The quotient obtained by dividing mental age by chronological age is called the intelligence quotient, or I.Q.

A satisfactory measure of physical development has long been sought; and different workers have used different measures, as indicated in the historical review. We were impressed by the value of teeth and body hair as indices of physical development. Weight seemed to us too dependent on social, nutritional and familial differences to be very useful. Stature, as less dependent, appears more useful. Finally it was decided to use the average derived from the standard age of a number of physical and physiological traits or events as follows:

1. Age at dehiscence of each tooth.
2. Age at cutting of each tooth.
3. Age at each stage in development of pubic hair.
4. Age at each stage in development of axillary hair.
5. Age of given stature.
6. Age of given sitting height.
7. Age of given relative span.
8. Age of given increment of stature.

For each of the foregoing eight traits a standard age series has been worked out like the standard age-series of the Binet scale. The method of origin of each is described below.

1. Tooth Dehiscence

At birth the typical child has the crowns of the 20 permanent teeth practically completed and these are erupted on the average in Nordics at the ages as shown in the Table 1. This table is inserted to complete the story of tooth development. None of our children were of these ages. This table is based, in part, on Tables 1 and 2 of Cattell ('28, p. 9), taken in turn from Legros and Magitot, and Thoma, and in part upon observations made on Hebrew infants at a "Home" in New York City (Boas, 1927). The series of Boas is admittedly of children whose physical condition is generally retarded.

TABLE 2

Age, in months, of just completed eruption of temporary or milk dentition in man

	LEGROS & MAGITOT. CATTELL, '28, P. 9 *Months*	THOMA. CATTELL, '28, P. 10 *Months*	BOAS, '27 (BOYS) *Months*
Med. Incisor (I)			
Inferior	6	6–8	12.1
Superior	10		13.3
Lateral Incisor (I_2)			
Inferior	16	7–9	16.8
Superior	20		16.6
Canine			
Inferior	30–32	17–18	22.0
Superior	30–32		22.3
1st Molar (M)			
Inferior	24	14–15	20.3
Superior	26		20.3
2d Molar (M_2)			
Inferior	28	18–24	28.0
Superior	30		28.9

The milk dentition is replaced by the permanent dentition usually at between 6 and 15 years. The arrival of the permanent tooth is usually preceded by the casting off of the corresponding temporary tooth. Dehiscence is not an event that occurs at one instant and in all children at the same age. It is a gradual and prolonged process the beginnings of which are already laid down (as germs of the permanent teeth) at birth. This prolonged process has been considered especially by Wuorinen ('26) in his study of tooth replacement among the Finns. He recognizes the following seven stages:

1. The milk tooth rests in its proper place.
2. Of the milk tooth only the root is in place.
3. The milk tooth is out, the persisting tooth is invisible.
4. The milk tooth is out, the persisting tooth is invisible and there is no place for it in the dental series; *i.e.*, space due to the dehiscence has closed up.
5. The crown of the persisting tooth has just penetrated the gum.
6. The greater part of the circumference of the tooth crown is visible in the opening of the gum.

7. The crown is completely broken through.

A tooth is called completely broken through as soon as, with closed jaws, it is in contact with its antagonist or as soon as its crown has completely arisen from the gum. Stages 3 and 4 (practically stage 3) represent what in this paper is designated as dehiscence.

TABLE 3

Age, in years, of dehiscence of temporary dentition in boys

	UPPER TEETH					LOWER TEETH				
	I_1	I_2	C	M'_1	M'_2	I_1	I_2	C	M'_1	M'_2
Wuorinen, '26, p. 34–47. Finns	6.5	7.8	10.9	9.8	10.4	5.6	6.7	9.4	9.3	10.4
*Gebhardt, '25. Italian.	7.4	8.2	11.6	9.6	10.6	6.8	7.9	10.9	9.9	10.1
†Sicher & Tandler, '28, pp. 162, 193	7.5	8.5	12.5	10.5	11.5					
Cattell, '28, p. 9. Ages weighted (chiefly Nordic)	6.5/7	7/8	11/12	9.5/10	10/11	6/7	7/8	11/12	9/10	10/11
Cattell, '28, p. 10, from Thoma	6/7	7/8	11/12	9/10	11/13	6/7	7/8	11/12	9/10	11/13
This study	6.5	7.8	10.9	9.8	10.3	5.7	6.7	9.4	9.3	10.5

* From Wuorinen, '26, p. 15.

† Approximate, from text figures.

In Table 3 are given data on age of dehiscence of temporary teeth, as published by four authors. Also on the bottom line are given the ages adopted as "standard ages" for dehiscence of temporary teeth. Our standard ages (based in part upon Wuorinen's ('26) work and experience at the Brooklyn Orphan Asylum, children of Nordic stock) run, on the average, about 6 months younger than Cattell's ('28) series and deviate somewhat more than that in the case of the canines. This low standard age for tooth dehiscence would have an effect in tending to diminish the P.Q. of the boys, ages 10 to 11 say by $\frac{1}{6}$ year.

2. Eruption of Permanent Dentition

The eruption of the permanent tooth is a prolonged process. Wuorinen ('26, p. 33) recognizes the following stages:

a. The crown has just broken through the gum.

b. The crown is half through.

c. The crown is fully broken through.

d. The tooth is fully out and thus in occlusion with its opposite—when the opposite is also fully developed.

The periods of visible eruption of the different teeth—that is, from stage *a* to stage *d*, above, are given by Wuorinen ('26, p. 79) for boys as follows:

	UPPER JAW							LOWER JAW						
	I_1	I_2	C	P_1	P_2	M_1	M_2	I_1	I_2	C	P_1	P_2	M_1	M_2
Years	0.6	1.0	0.8	0.9	0.3	0.3	0.6	0.5	0.8	0.8	0.6	0.5	0.2	0.5

Thus, on the average, the upper lateral incisor is erupting during a period of one year. In my own observations in the case of single teeth as much as two years have elapsed between dehiscence of a milk tooth and the full eruption of the corresponding permanent tooth.

The events of dehiscence of temporary teeth and eruption of permanent teeth constitute a series of events extending from 6 to 12 years. The series is thus an admirable time marker for physical development. Cattell ('28) has discussed fully, and made good use of this series in her paper "Dentition as a measure of maturity". But in any year the correlation between mental age and dentition is not much greater than the probable error (Cattell, '28, p. 82).

The ages of eruption of the teeth of the permanent dentition, as found by various authors, and the standard ages adopted by us in this study are shown in Table 4. The ages adopted in this study, shown in the bottom line, agree closely with those adopted by Cattell.

3. Development of Permanent Pubic and Axillary Hair

At about the age that the second permanent pre-molar is erupted the terminal hairs of the pubic region begin to appear. The development of hair in the pubic region passes through several stages, as Crampton ('08, p. 143) has pointed out. He recognizes three stages: (1), only fine hairs on pubis; (2) rapid growth of the fine hair on the pubis and its acquisition of pigment; (3) appearance of the characteristic kink in the hair; about 6 months subsequent to (1).

In this study a somewhat different system was used, partly because the kinking of the hair is not a universal phenomenon. Thus in Mongoloid dwarfs the pubic hair does not kink. In place we adopted

the standards based upon observations made on the Brooklyn Orphan Asylum children.

The pubic hair in its development passes through a series of stages.

a. The pubis is covered by unpigmented down like that over the front of the trunk, usually about 2 or 3 mm. long.

TABLE 4

Age, in years, of eruption of permanent dentition, from various authors, boys

	UPPER JAW						
	I_1	I_2	C	P_1	P_2	M_1	M_2
Suk, '19, p. 374	7 to 8	8 to 9	11 to 12	10 to 11	10 to 12	6 to 7	12 to 14
Nadeshdin, '25, p. 274, from Unglaûbe	5.2/8	6.5 to 9	9 to 12½	9 to 12½	9½ to 15	5 to 7½	10 to 14
Nadeshdin, '25, p. 274 from Lacassagne	8	9	12	10	.....	7	15
Bean, '14	7 to 8	8 to 10	11 to 12	10 to 11	10 to 12	6 to 7	12 to 14
Gebhardt, '25 (Italian)	7.5	8.6	11.5	10.4	11.0	6.7	
Wuorinen '26, p. 83	7.2	8.4	11.8	10.6	11.7	6.3	12.5
Hellman,' 23, from Cattell, '28, p. 12	7.7	9.0	12.2	10.6	11.1	6.6	12.7
Cattell, '28, p. 13, Röse, '09	7	8	12	10	11	6	12
Cattell, '28, p. 61, (own)	7.3	8.4	11.5	10.3	11.1	6.3	12.2
This study	7.4	8.5	11.5	10.4	11.0	6.4	12.3

	LOWER JAW						
	I_1	I_2	C	P_1	P_2	M_1	M_2
Suk, '20, p. 21	6 to 7	7 to 8	10 to 11	10 to 12	11 to 12	5 to 6	11 to 14
Nadeshdin, '25, p. 274, from Unglaûbe	6 to 9	7 to 10	10 to 14	8½ to 13	9 to 14	5 to 8	10.5 to 14.5
Nadeshdin, '25, p. 274, from Lacassagne	8	9	12	10	.....	7	15
Bean, '14	6 to 7	7 to 9	11 to 12	10 to 12	11 to 12	6 to 7	11 to 14
Gebhardt, '25 (Italian)	6.7	7.9	11.3	10.5	11.7	5.9	
Wuorinen, '26, p. 83	6.3	7.3	10.7	10.9	11.5	6.0	12.0
Hellman, '23, from Cattell, '28, p. 12	7.0	8.2	11.1	11.3	11.9	6.6	11.8
Cattell, '28, p. 13, Röse, '09	6	7	11	11	12	6	12
Cattell, '28, p. 61, (own)	6.3	7.6	10.7	10.6	11.3	6.2	11.7
This study	6.4	7.5	11.0	10.8	11.4	6.1	11.8

b. The down begins to grow rapidly, but still is fine and not deeply pigmented (except in deeply pigmented races). Hairs of this intermediate stage are called semi-terminals. Their length is estimated at from 4 to 10 mm.

c. The further development of the semi-terminals leads to coarse, fully pigmented, usually wavy, terminal hair, not over 25 mm. long; at first, rather sparse (sometimes two to five hairs will grow to 30 mm. before the others start) and later, as more follicles come into action, of full density (stage *d*).

e. Finally we have the fully developed dense, frequently curly, hair over 25 mm. long.

The standard ages assigned to each of these stages are as follows: *a*, under 12 years; *b*, 12 years; *c*, 13 years; *d*, 14 years; *e*, 15 years and over.

Axillary hair usually appears 2 years later than the pubic hair. The stages recognized and their standard ages (derived empirically from the B.O.A. series for boys) are as follows:

a. down, 2–4 mm., under 14 yrs.,

b. down or semi-terminals, 5 to 10 mm., 14 yrs.,

c. terminals, 10 to 24 mm., sparse, 16 yrs.,

d. terminals, 25 mm. and over, dense, 18 yrs. and over.

The stages of pubic and axillary hair extend the age series from 12 to 18 years and are extremely valuable physiological age markers.

4. Anthropometric Markers

After consideration of the whole range of available anthropometric data it was decided to use the following.

a. Age at vertex height. With the aid of numerous data taken from Baldwin (1921) and Hastings (1901) the stature of males, ages 3 to 20 years, mostly of Nordic stock, was plotted as a standard (Table 5). Then the stature found in a boy of a given chronological age was compared with this standard, to find his physiological age, so far as this element is concerned.

For the 8 Jews our adult standard is probably about 3 to 4 per cent too tall; and for the 11 Italians probably about 4 to 5 per cent. This defect in our standard is not very important; so far as it goes it tends to diminish the correlation between physical and mental development.

b. Relative span. As is well known the young boy has a span that is less than his stature. At 7 years the span, on the average, equals stature and by 13 years, when the appendages have finished their

TABLE 5

Standard vertex height (stature) for American males

EXACT AGE IN YEARS	STATURE *mm*	EXACT AGE IN YEARS	STATURE *mm*
3	899	12	1455
4	964	13	1480
5	1020	14	1550
6	1152	15	1618
7	1217	16	1667
8	1263	17	1714
9	1308	18	1726
10	1366	19	1728
11	1407	20	1729

maximum growth, the span is about 101.7 per cent of stature. Table 6 of standard spans (based on B.O.A. experience) was drawn up.

TABLE 6

Standard Relative Span at various ages

YEAR OF AGE	5	6	7	8	9	10	11	12	13
Rel. span	99.0	99.5	100.0	100.5	101.0	101.2	101.4	101.6	101.7

It not infrequently happened that boys were measured whose relative span lay outside of the limits of 99 and 101.7. In such cases the relative span was simply not used for comparison. Span is indeed the least satisfactory of the age markers, partly because it is difficult to measure precisely.

c. Relative sitting height. As the arms grow faster than stature in younger years so also do the legs. Consequently the relative sitting height (sitting height ÷ stature) decreases, on the average, up to 13.5 years. The standards shown in Table 7 were employed.

TABLE 7

Relative sitting height. Males

YEAR OF AGE	4	6	7.6	8.5	9.7	12	13.5
Rel. Sitting Ht.....	57	56	55	54	53	52	51.5

Here, again, ratios found to lie beyond these limits had to be rejected for the comparison. The error in this determination is fairly high.

d. Annual increments in total body weight. The standard employed on this scale is the same as that described by the senior author (1926, Fig. 2). From about 5 or 6 years to 15 the increments of total body weight increase with age. After 15 years they decrease. The actual standard increments employed are shown in Table 8.

TABLE 8

AGE IN YEARS	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Ann. increment Weight (Kg.).	1.8	1.85	1.9	2.1	2.3	2.4	2.5	2.7	3.2	3.8	4.8	5.3	4.8	3.5	1.9	0.5

Naturally caution and judgment have to be used in applying the right-hand end of this table. For example, an increment of 4.8 kg. corresponds either to an age of 14 yrs. or 16 years. The table can be used, however, since we have successive annual measurements of the same boy and can infer whether he has passed the maximum corresponding to 15 years or not.

The difficulties in utilizing this scale arise from the fact that annual increment in weight is a very sensitive measure, much influenced by environmental conditions. Thus it has occasionally happened that in a boy at 12 years the increment was 0.5 kg. Obviously this increment could not be used. The normal increment had been modified by a severe illness. Usually, however, in a case like this the increment of the following year is excessive, e.g., 4.5 kg. Under these circumstances the increment for the two years is added and about 48 per cent of the sum taken as the annual increment of the earlier period and 52 per cent as that of the later period. This treatment was all that was necessary in nearly all cases of temporary illness.

In incorporating the data of increment of weight from the preceding date of weighing into our totals of age scores it seemed desirable to take for any age the average of the two increments above and below the date of measurement; or if only the increment on one side of the date of measurement was known to incorporate half of this increment as a unit of half value in securing the average score.

5. Computation of Physical Age

After the physiological, or standard, age had been found for all of the foregoing events that were occurring in the boy at the time of observation the sum of such ages was found and, dividing by the number of events, the average physical age at the time of observation was secured. Then this mean physical age was divided by the chronological age of the boy and the resulting quotient was taken as the physical quotient of the individual at the time of observation.

It may be well at this stage to illustrate the process of determination of the physical quotient by giving in full the findings in a particular individual, as entered on the schedule used in this case. The case selected as No. 22 (W.B.), a boy largely of English stock who was examined at periods about one year apart when he was aged 9 years, 7.7 months; 10 years, 7.3 months; 11 years, 7.8 months; 12 years, 7.3 months; and 13 years, 7.5 months respectively.

Table 9 shows that at the first year of observation, at about 9.6 years, he was cutting one or both of the upper premolars, an event that usually occurs at 10.4 years; so that, in this respect, he was precocious. His vertex height was that of the average Nordic of 8.3 years. In his relative span he was still more retarded—like a boy of 7.1 years; and in his relative sitting height (which is closely the reciprocal of relative span) he is like an average boy of 7.5. His increment of weight from 9.6 years to 10.6 years is about 3.5 kilos, corresponding to the average increment of a boy of 12.5 years. Since this increment is determined for the year between two observations, half the increment, with the weight of one-half, is considered as the rate of increment in weight at 9.7 years. The total of the 4 data of the time of examination and ½ the increment in weight is 39.6, which divided by 4.5 gives 8.8 as the boy's physical age at 9.6 years. The ratio 8.8/9.6 is .91 which is W.B's physical quotient at 9.6 years. This may be compared with his IQ. (.66) at the same age. In general, the physical quotients run higher than the intelligence quotients. The boys are less variable in PQ. than IQ and physically nearer the standard than mentally. These differences result from the fact that these boys in an institution for the feeble-minded were selected primarily for their low IQ. If their PQ. is below normal, it is because of a correlation between IQ. and PQ. The size of this correlation is the subject of our investigation.

To continue with W.B., at 10.6 years he was erupting no teeth. In the pubic region the down was developing into "semi-terminals",

TABLE 9

Sample of table used in calculation of physical quotient

Name W.B. Born 1914 Race U. S. (Eng.) Series D. 22

No. of Exam.....	1st.	2nd.	3rd.	4th.	5th.	6th.	7th
Date of Exam.....	II. 22. '24	II. 11. '25	II. 26. '26	II. 11. '27	II. 17. '28		
Chron. Age y: m: d...	9:7:22	10:7:11	11:7:25	12:7:10	13:7:16		
Mental Age.....	7.0	7.6	8.0	8.8	8.1		
I. Q.....	.66	.68	.67	.67	.63		
Tooth Dehiscence							
I_1 up (6.5)							
low (5.7)							
I_2 up (7.8)							
low (6.7)							
C up (10.9)			10.9				
low (9.4)			9.4				
P_1 up (9.8)							
low (9.3)			9.3				
P_2 up (10.3)							
low (10.5)							
Tooth Cutting							
I_1 up (7.4)							
low (6.4)							
I_2 up (8.5)							
low (7.5)							
C_1 up (11.5)			11.5	11.5			
low (11.0)				11.0	11.0		
P_1 up (10.4)	10.4						
low (10.8)				10.8			
P_2 up (11.0)				11.0			
low (11.4)				11.4			
M_1 up (6.4)							
low (6.1)							
M_2 up (12.3)				12.3			
low (11.8)			11.8				
Pubic Hair							
S. t. 5–10m 12		12	12	12			
T 10–25 m sp. 13					13		
T 10–25m den. 14							
T 25m + dense 15							
Axillary Hair							
D or st. 5–10m 14							
T 10–24 m sp. 16							
T 25 m + den. 18							

short unpigmented hairs of somewhat firmer texture than "down". The state of development of this hair is that of the average 12 year old boy. In this respect W.B. is precocious. His vertex height is that of a boy of 10.0 years; his relative span is like that of a boy 8.1 years

TABLE 9 (continued)

Vertex Height								
Relative Span		8.3	10.0	10.0	11.1	13.1		
99.0	(5)							
99.5	(6)							
100.0	(7)	7.1						
100.5	(8)		8.1					
101.0	(9)							
101.2	(10)			10.0				
101.4	(11)							
101.6	(12)					12		
101.7	(13)				13.0			
Relative Sitting Height ♂ %	Yrs.							
57	(4)							
56	(6)							
55	(7.6)	7.5						
54	(8.5)		8.0	8.7	8.2			
53	(9.7)					9.0		
52	(12)							
51.5	(13.5)							
Annual Increments in Weight. Kg.								
1.8	(4)							
1.85	(5)							
1.9	(6)							
2.1	(7)							
2.3	(8)							
2.4	(9)							
2.5	(10)							
2.7	(11)		11.0		11.1			
3.2	(12)	12.5		12.0				
3.8	(13)							
4.8	(14)							
5.3	(15)							
4.8	(16)							
3.5	(17)							
1.9	(18)							
0.5	(19)							
Divisor and Totals		4.5 39.6	5 49.9	10 105	11 123.9	5.5 63.6		
Avg. Scores		8.80	9.98	10.5	11.26	11.56		
P.Q.		91.3	94.2	90.1	89.2	84.8		
I.Q.		66	68	67	67	63		

of age and his relative sitting height is nearly the same. As for increment of weight, his age is taken midway between 12.5 and 11.0 years, or at 10.75 years. We have, thus, the standard age of five events

that are happening on this boy at 10.6 years. The average of these standard ages is 9.98 and the physical quotient is .94, a slight increase over last year's physical quotient, also his IQ, has increased from .66 to .68.

It will not be necessary to show in further detail how the remaining PQ's were computed for this boy. Attention may be directed to this point, that where the same event, of tooth cutting, occurs in two successive years this is either because the given tooth on one side cut a year earlier than the corresponding tooth on the other side, or else one or both paired teeth were over a year in "cutting",—a condition which we have noted on several occasions.

The series of PQ's and IQ's for W.B. may now be placed side by side in Table 10 as derived from Table 9.

TABLE 10

Relation between PQ and IQ in boy W.B.

AGE, YRS.	9.6	10.6	11.6	12.6	13.6
PQ	91	94	90	89	85
IQ	66	68	67	67	63
PQ-IQ	25	26	23	22	22

One sees in this case a parallelism between PQ and IQ which is striking. But so close a parallelism is not usually found, otherwise the coefficient of correlation between the two quotients would be between 90 and 100 per cent. One sees that the PQ fluctuates slightly in this case—a little more than the IQ; and that both are tending to diminish with age. In how far this is a usual event will be considered later.

C. Results on Correlation between Mental Age and Physical Age

From the data on chronological, mental and physical ages, there are computed the correlation coefficients for each of the chronological ages. The results are given in Table 11.

From this table it appears that the mean physical age lags more and more behind the chronological age as the latter advances, and that mean mental age does so still more. In fact, in passing from 9.5 years to 13.5 years, or 4 years altogether, the mean mental age increases less than 1 year; while the physical age increases slightly over 2 years. Incidentally the standard deviation of the mental age increases

TABLE 11

Showing the physical age, mean mental age and their standard deviations and the correlations between the PA and MA for each chronological age, 9.0–13.99 years

Chronolog. Age	N.	Mean MA.	σMA.	Mean PA	σPA	Coefficient of Correlation
9.0– 9.99	49	6.010 ± 0.175	1.819 ± 0.124	8.949 ± 0.108	1.126 ± 0.071	0.237 ± 0.091
10.0–10.99	55	6.355 ± 0.167	1.834 ± 0.118	9.536 ± 0.079	0.872 ± 0.056	0.253 ± 0.085
11.0–11.99	47	6.606 ± 0.194	1.970 ± 0.137	10.117 ± 0.085	0.864 ± 0.060	0.374 ± 0.085
12.0–12.99	49	6.724 ± 0.206	2.140 ± 0.146	10.684 ± 0.082	0.851 ± 0.058	0.258 ± 0.090
13.0–13.99	39	6.782 ± 0.241	2.229 ± 0.170	11.295 ± 0.134	1.245 ± 0.095	0.483 ± 0.074

regularly from 9.5 to 13.5 years. The coefficient of variation increases slightly from about 30 p.c. to 33 p.c. The standard deviation of the physical age, on the other hand, seems to be more irregular; it diminishes from 9.5 to 12.5 and rises in the 13 year group. The great variability of boys at 13 and 14 years is well known; this is associated with the adolescent spurt in development.

The right hand column gives the coefficients of correlation between mental and physical age. It is largest at 13 years, reaching the point 0.48 ± 0.07, which is a fairly large correlation, like that of the resemblance between brothers as computed by Pearson ('02k). Except for the slump at 12 years the correlation tends to rise from 9 to 13; and this rise is undoubtedly in part due to the increase in available physical data as one advances in years from 9 up to 14, in part to increase of variability of MA and PA with age.

It is remarkable that, though mental age lags so far behind physical age, in any one year of age the correlation between them should be so considerable; amounting under the most satisfactory conditions to nearly 50 per cent.

Finally the correlation was computed between the Physical Quotient and Mental Quotient. These are ratios, having the age as a denominator in both cases. It seemed possible that a spurious correlation might be introduced by this fact. The results are shown in Table 12.

TABLE 12

The coefficient of correlation between PQ and IQ and other constants for 61 to 71 boys measured each year at Letchworth Village

Year	No.	Mean PQ	σPQ	Mean IQ	σIQ	Coefficient of Correlation
1924.....	61	95.21 ± 1.00	11.62 ± 0.71	59.96 ± 1.74	20.12 ± 1.23	0.289 ± 0.079
1925.....	71	91.37 ± 0.82	10.25 ± 0.58	60.04 ± 1.39	17.46 ± 0.99	0.358 ± 0.072
1926.....	61	89.80 ± 1.02	11.79 ± 0.72	56.84 ± 1.35	15.67 ± 0.96	0.363 ± 0.075
1927.....	66	88.64 ± 0.81	9.72 ± 0.57	57.05 ± 1.38	16.82 ± 0.99	0.415 ± 0.069
1928.....	62	87.18 ± 0.76	8.88 ± 0.52	54.60 ± 1.31	15.33 ± 0.93	0.490 ± 0.065

From Table 12 it appears that both PQ and IQ tend to decrease with advancing age of the boys, despite repetition of tests. The variability declines in about the same degree. The coefficients of correlation vary from 0.29 to 0.49, running slightly (but hardly significantly) lower than the coefficients of correlation between PA and MA as shown in Table 11. It is not clear that a spurious correlation has been introduced by the use of the indices.

The significance of the variation in the coefficients as found by us may be briefly discussed. We have the distinct impression that it is, in part if not chiefly, due to the fact that at the later years both mental and physical ages are more variable (Table 11) and in consequence any relation between the two is, as it were, magnified, despite the fact that r tends to decrease with increase of σ; for $+r$ increases with the total positive x and y deviations from the mean.

In the case of the quotients, since chronological age increases faster than physical or mental age, the quotients and their σ's tend to diminish with age and that diminution is so marked as to raise the value of r. From this point of view the coefficients of the later ages, about 13 years, are probably the most significant as a measure of the true relation between physical and mental development.

D. General Discussion

The development of the normal child not only passes through such and such a series of changes but it passes through them at a certain rate. This rate is determined by the cooperation of internal and external factors. Now, in the development of the body many processes occur simultaneously and these are ordinarily carried along together. Such are in later life the development of the skeleton, of epidermal organs and teeth, of endocrine organs, of reproductive organs, of nervous system. The nervous system, to repeat, must go through the same maturing process as the reproductive organs. If the latter are retarded in maturing the child remains retarded, infantile, in his reproductive system; and also in his nervous system, in his intellect.

Not only does the process of development proceed at different rates in different individuals, but also the correlation in the development of all organs is not perfect. Bilaterally symmetrical organs, like the two arms or the two legs, usually develop at the same rate and the correlation between them at any stage of development is

remarkably high. Also there is a close correlation in the rate and degree of development of the corresponding bilaterally symmetrical teeth. In the dental series, however, the correlation in times of appearance of corresponding teeth of the upper and lower jaw is less complete than in the corresponding teeth of the two sides of the same jaw. And, indeed, certain teeth, like the upper lateral incisors, may fail altogether to develop or one pair may be rudimentary. And so it is in the interrelation of development of other organs of the body, some may be more retarded than others and some may partly or wholly fail of development.

In other words, the correlation in development of non-homologous organs is less close than that of symmetrical homologous organs. And it is a matter of measurement to determine what the correlation in any case is.

In the present paper an attempt has been made once again, on a somewhat new basis, to find the correlation between the development of the cerebral cortex, upon which, it is believed, mental development depends, and the development of a combination of physical traits—weight, stature, proportions, teeth and body hair. Some observers have concluded that the correlation between physical and mental development is low; others that it is fairly high. A review of the literature on the subject leads to the conclusion that the very low correlations are found where the range of intelligence is, by selection of material, small and where development is nearly finished; that a high correlation, on the other hand, is found where there is a great range in mental development and the developmental processes, in general, are proceeding most rapidly, as during the three or four years preceding and following puberty. When computed for that period it appears that the correlation between physical and mental development is about the same as the correlation in stature of two brothers, under the best conditions, namely, about 0.50.

While the causes of feeble-mindedness are varied and the corresponding types diverse, one fact stands out clearly: the feeble-minded have not developed properly; have, in general, developed insufficiently or inadequately. When we know more about the internal factors that control development we shall be better able to judge, presumably, which factors are absent in a given case; or what external favoring conditions are inadequate to permit the internal factors to do their work. It is hoped that in the next few years we shall have unraveled these internal factors, first of all through experimentation on animals,

and also by isolation of the hormones that promote growth. When we are able to identify with certainty the growth-promoting hormones, to measure their amount in the blood and to supply any insufficiency artificially through injection into the blood stream then we shall have added an important step to our knowledge of the causes of physical and mental dwarfness and to our ability to control or prevent such dwarfness.

Insufficiency of development has been ascribed to many causes. In the case of the cretin the secretions of the thyroid are insufficient, so that if these be added during the early growing stages the chain of causes that promote growth is completed again. In other cases, as Cushing has so clearly shown, hypopituitarism may result in infantilism and its accompanying general insufficiency of growth. Other accompaniments of infantilism that have been noted are preadolescent tuberculosis, cardiopathy, chronic intestinal infection. Jansen ('21, p. 7) thinks that dwarfness is due to a feebleness of growth traceable to the fatigue of the mother during pregnancy, so that dwarfs are often the last of a long series of pregnancies.

While, on the whole, there is a fairly close relation (50 per cent) between physical and mental dwarfness yet the two are often independent. No. 111 with an IQ that had decreased from 86 to 74 has a PQ of around 107; he is exceptionally tall. No. 64 with an IQ that has decreased from 87 to 71 has a PQ that has varied from 77 to 87; he is small for his age.

In the group of chondystrophic dwarfs, as Maas ('23, p. 84) points out, there is no clear correlation between constitution and psychic traits. Some ateliotic dwarfs have at least normal mental development. Mongoloid dwarfs, on the other hand, are usually mentally underdeveloped, frequently extremely so.

These boys differ also in the degree of infantilism they show. There are many sexually underdeveloped dwarfs (infantiles); but also some dwarfs whose external genitalia develop at the normal time; there are, however, few if any rapidly growing boys whose genitalia are retarded in development though in Cushing's (1912) giant the hirsuties were underdeveloped.

These considerations merely support our general findings, namely, that, by and large, the correlation between mental and physical development is not complete; is, indeed, as shown above, about 50 per cent.

II. CONSTANCY OF THE IQ

The mental age, as determined by the Binet test, is a function of chronological age. If a defective boy of 8 has a mental age of a 6 year old boy his mental development is $\frac{6}{8}$th that of a normal boy. This ratio, expressed as .75, is his Intelligence Quotient, IQ. As the defective boy gets older his intelligence advances. It has generally been assumed that it advances more slowly than chronologic age; but keeps the same ratio. Thus, at 12 years of age, it is expected that our defective boy will have the mental age of 9 years; and again the IQ of .75. The intelligence, it is thought, is fully matured at 16 years. After that, while the chronological age continues the mental age does not. Accordingly the IQ, which would constantly decrease with age, has ordinarily no meaning beyond 16 years.

Since, by definition, the mental age is measured by the performance of a "normal" child of a given chronological age, for such a normal child the relation MA/CA must be constant. If it is not constant it is because the scale of mental ages is not adapted to the population that is being measured. The MA's of Binet were based on experience with French children. The tests employed involved the use of language. If, for example, the French children have either a superior gift of language and better training in it than (let us say) American children, then the MA's of the latter would fall behind the chronological ages and their IQ would decrease. On the other hand, let us assume a group of boys of literary or scholastic stock who from an early age had received special training in language and shown an hereditary capacity for it; then this group might well pass from one standard "mental age" to another more rapidly than their chronological age advanced. Consequently for them the IQ would be constantly increasing. The problem can not, therefore, be properly formulated: Is the IQ constant? but rather: Does the IQ remain constant in the population studied? Or does it depart, on the average, from the standards set by Binet or his successors?

To answer the question the set of graded tests, like the Binet tests, must be applied in successive periods of a year or a half year, to the same children. In practice, the eight-year old child is given the 6-year, the 7-year, the 8-year and the 9-year test to determine his mental age. To the same child are given, a year later, when he is now 9 years old, the 7-year, the 8-year, the 9-year and the 10-year test. However, the situation is now changed. A year ago the 6-, 7-, 8- and 9-

year tests were all novel; now the 7-, 8-, 9- and 10-year tests are more or less familiar as he has had them once already, a year ago; so it is to be expected that he will show greater proficiency in them, just on account of this familiarity, and will proceed further and get a higher IQ than he did the first year. Accordingly the IQ may be expected to rise slightly in successive trials on the same person. Cattell and Gaudet (1930) have shown reason for concluding "that the factor of practice is important, perhaps the main cause of the excess of gains over losses."

It is not our purpose to review the entire considerable literature on the constancy of the IQ, but some of the findings may be mentioned. Thus Terman ('18) testing 435 normal children 5 to 12+ years of age, with an interval between tests of less than 1 to more than 5 years found a medium increase of +1.78 in the IQ of the later measurements. Wallin ('21) found, on the whole, a slight increase of IQ on the second measurement. Rugg-Colloton ('21) measuring 137 school children a second time at approximately a year later than the first test found a medium increase of 1.6 points in the IQ. Rugg ('25) gave the Stanford Revision test twice to each of 114 grammar school children (of considerable range of intelligence) and found that the average of the increases in IQ was 1.9, while the average of the decreases was only 1.2. Thus increases prevailed. The greatest change for an individual was 11 points. On the other hand, Rogers, Darling and McBride ('28) measuring the same children after the lapse of 1 to 3 years found that 75% at the second examination gained IQ's within 10% of those obtained on the first examination. Apparently the interval was too long to permit the carrying over of the earlier experience. Also if the first test be replaced the second time by quite a different one the increased IQ does not appear (Cattell and Gaudet, '30).

With feeble-minded children the result is somewhat different. Thus Kuhlman ('21) reexamined 639 feeble-minded children over a period of ten years and concluded that the IQ decreases with age. For border line cases this decrease amounts to 2.19 points per year; for morons 1.21; for imbeciles 1.04; for idiots 0.37. He concludes: "The IQ decreases with age and decreases more for higher than for lower grades." Presumably he is referring to absolute decrease; relative decrease would be more to the point, since there is less opportunity (or range) for an IQ of 25 to decrease than one of 75.

Poull ('21) compared the IQ of 126 inmates of Randall's Island 4 to 28 years of age and obtained IQ's of 20 to 90. The intervals between first and last tests was 0.5 to 3 years. The average difference

in IQ between first and second tests was +1.28. The conclusion is drawn that the subjects did not deteriorate as a group. The criticism may be suggested that all persons over 18 to 20 years of age should have been excluded from the averages.

Chipman (1929) reports on a study of 1751 cases at the Walter E. Fernald State School at Waverly, Mass., in which the IQ's at entrance have been compared with present IQ's. He finds a constancy of the IQ in 79% of cases. He also finds that use of 14 as age of mental maturity and a basis for computing IQ destroys its validity as diagnosis.

Correlations between IQ's of first and second tests have been found by various authors of between 0.72 to 0.95, and generally above 0.80. The variables are, however, so great that the results are not precisely comparable, but in general the stability of the I Q with normal school children of the United States indicates that the standards for M A, for the different ages, are tolerably satisfactory.

In our developmental series we have 275 yearly intervals between measurements of the 70, more or less, boys. Of first annual intervals there are 73 cases, of 2nd, 64 cases, 3d, 64 cases, 4th, 59 cases and 5th 15 cases. Altogether, the number of changes of each size in the 275 cases is as shown in Table 13.

TABLE 13

Intervals in IQ determination

Earlier—later grade	1st	2d	3d	4th	5th	Total
+19 to +15			1			1
+14 to +10	2	2	2			6
+ 9 to + 5	5	4	6	5	1	21
+ 4 to 0	25	7	34	10	8	84
− 1 to − 5	25	26	16	26	5	98
− 6 to −10	12	17	3	10	1	43
−11 to −15	2	7	1	4		14
−16 to −20	2	1	1	3		7
−21 to −25						0
−26 to −30				1		1
Totals.....	73	64	64	59	15	275
Mean difference.	−1.70 ± 0.45	−4.02 ± 0.51	0.75 ± 0.45	−4.02 ± 0.59	0 ± 0.62	−2.07 ± 0.25
σ.....	5.74 ± 0.32	6.01 ± 0.36	5.31 ± 0.32	6.68 ± 0.41	3.56 ± 0.44	6.15 ± 0.17

Considering first the totals as given in the right hand column it appears that the changes in IQ from earlier to later annual test average −2.073 + 0.250 points. The modal class of the differences is −5 to −1. The conclusion is drawn that, in these mentally inferior children,

there is, despite repetition of tests, a real diminution of the IQ in successive years, amounting to about 2 points per year. The coefficient of variation is about 3 per cent; precisely 2.97 per cent.

It may be of interest to consider how the average difference of the IQ changes as we proceed from the earlier intervals (on younger and less experienced children) to the later intervals. The third interval shows practically no change ($+0.75 \pm 0.45$); and the second and fourth show a clear decrease in IQ of 4 points. The meaning of this is not clear. Possibly by the third interval (fourth test) these defective boys had acquired a certain amount of improvement due to practice, which added to their mental age increment prevented them from showing the decrease in IQ that is otherwise apparent (omitting the fifth interval where the subjects were few and doubtless unconsciously somewhat selected).

We conclude, therefore, in accordance with the findings of others, that in feeble-minded boys there is a tendency for the IQ to decrease with age; though this decrease is slightly retarded by the effect of repetition of tests. In other words, the mental scale that was invented and based on normal children has too large intervals for defective or retarded children.

III. CONSTANCY OF THE PQ

The question arises as to the constancy of the PQ. Does this tend to remain at about the same point at the various ages? In some instances the constancy is striking; e.g. Nos. 1, 27, 37, 39, 40, 43, 66, 69, 72, 77, 81, 100. In other cases the PQ is constant except for 1 year, in which there is a large deviation, due largely to an insufficient number of data on which to compute PQ, e.g., Nos. 11, 17, 56, 63, 78, 112. On the other hand, we have cases such as Nos. 54, 68 and 79 which show great variability in the PQ. Such occasional variability is, indeed, to be expected. The important question is, is there any general tendency, as the boy becomes older, for the PQ to rise.

To test this the increments year by year were tabulated and the number of cases in which the increment from one year to the next was zero, or + in various degrees, or − in various degrees was listed. The result is given in Table 14.

From these figures we derive a total of 186 negative deviations and 125 positive deviations, or about 50% more of the former than of the latter. There is, thus, a tendency of the PQ to fall behind with advancing age and this is true of a population of boys few of whom at

TABLE 14

The frequency of the different classes of yearly increments

Yearly increments in PQ....	−30 to −26	−25 to −21	−20 to −16	−15 to −11	−10 to − 6	−5 to −1	0 to +5	6 to 10	11 to 15	16 to 20	21 to 25	26 to 30
No. of cases.....	1	3	10	19	61	92	86	28	6	1	4	

any time are over 16 and most of whom are 7 to 14 years of age—ages where physical development is most rapidly occurring. Indeed, with just these boys who are two or three years retarded the period of most rapid growth (as measured by weight) is not closed until after 17 or 18 years.

We must conclude, accordingly, that, useful as the PQ is, it is not constant but tends slightly to decrease (by one or more points) with age during the period 7 to 16 years.

This annual decrease is not the same at all ages. Thus (Table 15) in the second year, as compared with the first, there was an average decrease in PQ of 4.3 points; in the third year the decrease of PQ from the second was 1 point; in the fourth year under the third the decrease was 0.6 points; in the fifth year under the fourth, 2 points. The variability of the changes in PQ from one year to the next tends to decrease. The index of variability, σ, is 9.75 units in the first interval and only 5.56 units in the fourth.

TABLE 15

Showing the distributions in PQ in the successive measurements of approximately the same boys

Points of change	1st inter.	2d inter.	3rd inter.	4th inter.	5th inter.	
24 to 20	1	1	2			
19 to 15	0	1	1			
14 to 10	3	3	4		1	
9 to 5	8	10	5	5	6	
4 to 0	12	19	16	21	8	
− 1 to − 5	13	18	25	22	14	
− 6 to −10	16	14	12	10	10	
−11 to −15	5	4	2	5	4	
−16 to −20	5	2	1	1		
−21 to −25	2					
−26 to −30	2					
N.....	67	72	68	64	43	Σ 314
Avg.....	−4.27 ± 0.80	−0.97 ± 0.60	−0.58 ± 0.61	−2.38 ± 0.47	−2.42 ± 0.64	
σ.....	9.75 ± 0.57	7.53 ± 42	7.46 ± 0.43	5.56 ± 0.33	6.22 ± 0.45	

To sum up, the physical quotient tends in feeble-minded boys of 7½ to 14 years slowly to decrease with age from between 4 to 0.5 points

per year. The range of the changes in PQ for the different boys is greatest in the first interval and tends to diminish, just as the variability of the physical age does.

IV. SUMMARY OF CONCLUSIONS

A new method is utilized for measuring physical age; an average of data from teeth, secondary hair and anthropometric findings.

Repeated mental measurements and observations were made upon 78 white boys, at an institution for the feeble-minded, during (for the most part) six years. All were Nordics except 11 Italians, 8 Jews and 3 of unknown racial stock. The boys varied in age at time of measurement from 5.3 years to 19.7 years.

Correlations between physical age and mental age are found to vary from .24 to .48; and between the physical quotient and the intelligence quotient from .29 to .49.

Since chronological age increases faster than physical age or mental age the quotients and their σ's tend to diminish with age.

The correlation between physical and mental development is about the same as that between the stature of brothers; about .50.

The Physical Quotient tends to decrease slightly (about one or two points on the average) with age during the period 7 to 16 years.

The Intelligence Quotient of the feeble-minded boys tends to decrease slightly with age, probably more at first than later; though this decrease is slightly retarded by the effect of repetition of tests.

NOTE

A table, showing for each boy the racial stock, date of birth, dates (months and years) of examination, chronological age (C.A.), mental age, physical age, intelligence quotient and physical quotient has been prepared but it is too voluminous to publish. It has, however, been typed and the typed copy can be lent to any person who desires to make a special study of this table.

LITERATURE CITED

ABERNETHY, ETHEL M. 1925. Correlation in physical and mental growth. *J. Educ. Psychol.*, 16: 458–466, 539–546.

ARNOLD, F. 1916. Weights and school progress. *Psychol. Clinic*, 10: 33–39.

BAGLEY, W. C. 1901. The correlation of mental and motor ability in school children. *Am. J. of Physiol.*, 12: 193–205.

BALDWIN, B. T. 1921. The physical growth of children from birth to maturity. Univ. of Iowa Stud. Child Welfare, 1: 1–411.

———. 1922. The relation between mental and physical growth. *J. Ed. Psychol.*, 13 (4): 193–203.

BEAN, R. B. 1914. The stature and eruption of the permanent teeth of American, German-American and Filipino children. *Am. J. of Anat.*, 17 (1): 113–160.

BOAS, F. 1895. On Dr. William Townsend Porter's investigations of the growth of the school children of St. Louis. *Science*, N.S. 1: 225–230.

———. 1899. The growth of children. *Science*, N.S. 5: 570–573.

———. 1927. The eruption of deciduous teeth among Hebrew infants. *J. Dental Res.*, 7 (3): 245–253.

BOWDITCH, H. P. 1877. The growth of children. 8th Ann. Rept. State Board of Health, Mass. 8: 273–323.

CARMAN, ADA. 1899. Pain and strength measurements of 1507 school children in Saginaw, Mich. *Am. J. Psychol.*, 10: 392–398.

CARTER, T. M., and F. N. FREEMAN. 1924. A new measure of the development of the carpal bones and its relation to physical and mental development. *J. Educ. Psychol.*, 15: 257–270.

CATTELL, PSYCHE. 1928. Dentition as a measure of maturity. Studies in Educ. Psychol & Educ. Measurements. Harv. Monographs in Educ., 9: VIII + 91.

CATTELL, PSYCHE, and F. G. GAUDET. 1930. The inconstancy of the IQ as measured by repeated group tests. *J. Educ. Research*, 1–8.

CHRISTOPHER, W. S. 1900. Measurements of Chicago school children. *J. Am. Med. Assoc.*, 35: 618–623; 683–687.

CHIPMAN, C. E. 1929. The constancy of the intelligence quotient of mental defectives. *Psychol. Clin.*, 18: 103–111.

COZENS, F. W. 1927. Status of the problem of the relation of physical to mental ability. *Am. Phys. Educ. Rev.*, 32 (3): 147–155.

CRAMPTON, C. W. 1908. Physiological age, a fundamental principle. *Am. Phys. Educ. Rev.*, 13: 141–154; 214–227; 268–283; 345–358.

CUSHING, H. 1912. The Pituitary Body and Its Disorders. Philadelphia. 1–341.

DAVENPORT, C. B. 1926. Human metamorphosis. *Am. Jour. of Phys. Anthrop.*, 9: 205–232.

DE GIOVANNI, A. 1905. Studi di morfologia clinica. Milano. 2 vols.

DOLL, E. A. 1916. Anthropometry as an aid to mental diagnosis. Publ. Training School at Vineland, N. J. 8: 1–91.

GARRETT, H. E., and W. N. KELLOGG. 1928. The relation of physical constitution to general intelligence, social intelligence and emotional instability. *J. Exp. Psychol.*, 11: 113–129.

GATES, A. I. 1924. The nature and educational significance of physical status and of mental, physiological, social and emotional maturity. *J. Educ. Psychol.*, 15: 329–358.

GEBHARDT, JOHN C. 1925. The dentition of Italian children. *J. Am. Dental Assoc.*, 12 (5).

GILBERT, J. A. 1894. Researches on the mental and physical development of school children. Yale Univ. Psychol. Lab. Studies, 1892–6. 2: 40–100.

———. 1897. Researches upon school children. Iowa Univ. Studies in Psychol., 1: 1–39.

GODDARD, H. H. 1912. The height and weight of feeble-minded children in American institutions. *Jour. Nerv. & Ment. Dis.*, 39: 217–235.

GOULD, B. A. 1869. Investigations in the military and anthropological statistics of American soldiers. New York: Howard and Houghton. 1–655.

GRATSIANOFF, N. A. 1889. Materials for the Investigation of Physical Development in Childhood and Youth in Relation to Heredity and to Progress in School Work. St. Petersburg.

HASTINGS, W. W. 1901. A Manual for Physical Measurements—Boys. 1–95.

HEATON, K. L. 1925. Physical development of children of high and low mental ability groups. *Am. Phys. Educ. Rev.*, 30: 127–130.

HEIDBREDER, EDNA. 1926. Intelligence and the height-weight ratio. *J. Appl. Psychol.*, 10: 52–62.

JANSEN, M. 1921. Feebleness of Growth and Congenital Dwarfism. London. 1–82.

JOHNSON, B. J. 1925. Mental Growth of Children in Relation to Rate of Growth in Bodily Development. N. Y. 1–160.

KUHLMAN, F. 1921. The results of repeated mental re-examinations of 639 feeble minded over a period of five years. *J. Applied Psychol.*, 5 (3): 195–224.

LANDIS, M. H., BURTT, H. E., and J. H. NICHOLS. 1923. The relation between physical efficiency and intelligence. *Am. Phys. Educ. Rev.*, 28: 220–221.

LUTZ, W. F. 1924. The relation of physical to mental growth. *Psychol. Clinic*, 15 (5): 125–129.

MAAS, O. 1923. Beitrag zur Kenntnis des Zwergwuchses, II. *Ztschr. f. ges. Neurol. u. Psychiat.*, 86: 76–84.

MEAD, C. D. 1914. Height and weight of children in relation to general intelligence. *Ped. Sem.*, 21: 394–406.

———. 1916. The relation of general intelligence to certain mental and physical traits. Teachers' Coll. Pub. 1–117.

MURDOCK, K., and L. R. SULLIVAN. 1923. A contribution to the study of mental and physical measurements in normal children. *J. Am. Phys. Educ.*, 28: 209–215; 276–286.

NACCARATI, S. 1921. The morphologic aspect of intelligence. Columbia Univ. Contrib. to Philos. and Psychol., 27 (2): 1–44.

NADESHDIN, W. A. 1926. Zur Frage der Altersbestimmung Minderjähriger. *Ztschr. f. d. ges. gerichtl. Mediz.*, 8: 273, 274.

NORSWORTHY, N. 1907. Suggestions concerning the psychology of mentally deficient children. *J. Psycho-asthenics*, 12 (1): 3–17.

PERKINS, F. J. 1926. The relation of dentition to mental age. *Ped. Sem.*, 33: 387–398.

PORTER, W. T. 1893. The physical basis of precocity and dullness. *Trans. Acad. of Sci. of St. Louis*, 161–181.

POULL, L. E. 1921. Constancy of IQ of mental defectives, according to the Stanford Revision of Binet tests. *J. Educ. Psychol.*, 12: 323–324.

QUETELET, A. 1871. Anthropométrie, ou mesure des différentes facultés de l'homme. Bruxelles. 1–479.

ROGERS, A. L., D. DARLING, and K. MCBRIDE. 1928. The constancy of the IQ and the training of examiners. *J. Educ. Psychol.*, 19: 257–262.

RUGG, L. S. 1925. Retests and the constancy of the IQ. *J. Educ. Psychol.*, 16: 341–343.

RUGG, H., and COLLOTON, C. 1921. Constancy of the Stanford-Binet IQ as shown by retests. *J. Educ. Psychol.*, 12: 315–322.

SACK, N. 1893. Uber die körperliche Entwicklung der Knaben in den Mittelschulen Moskaus. *Ztschr. f. Schulgesundheitspflege*, 6: 649–663.

SCHUYTEN, M. C. 1902. Les variations de la force musculaire et le développement intellectual des élevès: summarized by A. Binet. *L'Année Psychol.*, 9: 448–449.

SEVERSON, S. O. 1922. The relation of the anatomical age to the chronological, pedagogical, and mental ages with special reference to sex differences. *Proc. & Addr. Am. Assoc. Study of Feeble-Minded*, 150–170.

SHELDON, W. H. 1927. Morphologic types and mental ability. *J. Personnel Res.*, 5: 447–54.

SHUTTLEWORTH, G. E. 1886. The health and physical development of idiots as compared with mentally sound children of the same age. *Proc. Assoc. Med. Officers of Amer. Institutions for Idiotic & Feeble-Minded Persons*, 1: 315–322.

SICHER, H., and J. TANDLER. 1928. Anatomie für Zahnärzte. Wien & Berlin. 1–415.

SMEDLEY, F. W. 1900. Report of department of child-study and pedagogic investigation of Chicago public schools. 46th Ann. Rept. Bd. of Educ., Chicago. 37–116.

SUK, V. 1919. Eruption and decay of permanent teeth in whites and negroes, with comparative remarks on other races. *Am. J. Phys. Anthrop.*, 2: 351–388.

TARBELL, G. G. 1883. On the height, weight, and relative rate of growth of normal and feeble-minded children. *Proc. Assoc. Medical Officers of Amer. Inst.*, 1: 188, 189.

TERMAN, L. M. 1919. The Intelligence of School Children. How Children Differ in Ability, etc.

VIOLA, G. 1905. La tecnica antropometrica a scopo clinico. Padova.

WEST, G. M. 1898. Observations on the relation of physical development to intellectual ability made on the school children of Toronto, Canada, *Science*, 4: 156–159.

WHEELER, L. R. 1929. A comparative study of the physical growth of dull children. *J. Educ. Research*, 20: 273–282.

WHIPPLE, G. M. 1914. Manual of Mental and Physical Tests. Baltimore. Vol. I, 1–365; 1915. Vol. II. 1–336.

WISSLER, C. 1901. The correlation of mental and physical tests. Psychol. Monographs, 3 (6): 1–62.

WUORINEN, T. A. 1926. Die Dauer des äusserlich wahrnehmbaren Durchbruchs der permanenten Zähne. Helsinfors. 1–91.

WYLIE, A. R. T. 1899. Investigation concerning the weight and height of feeble-minded children. *J. of Psycho-Asthenics*, 4 (2): 47–57.

———. 1903. Contribution to the study of the growth of the feeble-minded in height and weight. *J. of Psycho-Asthen.*, 8 (1): 1–7.

SOME ASPECTS OF PATERNITY IN SENDAI, JAPAN*

BY THOMAS J. LEBLANC

DIFFERENTIAL FERTILITY

With the exception of immigration and emigration, both of which in Japan are negligible, the social or economic character of the population is largely determined by the reproductive activity of the various groups that make up the population. This statement is qualified because it is recognized that social or economic groups are not fixed or clearly defined entities; there is considerable movement from the lower to the higher classes as well as a slight flow in the opposite direction, both of which depend upon the general character of the society and the stresses and strains set up by varying economic pressure. In addition there is a time element involved, since the reproductive activity of the present determines population characteristics not for the present, but for the immediate future, that is the next generation; but in general, the rate at which any given class, low, middle or high, on the basis of any definition, is reproducing, determines the proportion of that class as it occurs in the general population. This question of what is termed differential fertility has received more or less critical examination in many countries; it is, however, next to impossible to find satisfactory data bearing on the problem in spite of the fact that if differential fertility exists, the study of its operation is a matter of prime importance.

The general trend of the conclusions reached as the results of these studies is that there is a distinctly unfavorable differential fertility operating in most countries. The lower classes, or relative undesirables, are reproducing faster than the upper classes, or desirables, and this condition causes not a little concern. Whether or not this is actually true is open to question, since the conclusion is usually based

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on samples of college graduates, representing the desirables, and imbeciles, paupers, etc., as undesirables. While it is clear that imbeciles cannot be valuable citizens from either a social or an economic standpoint, on the other hand college graduates and persons found in standard compilations of famous people do not have a monopoly on the good, the true and the beautiful. Such samples represent only the extremes of society and any alarming tendencies found in such extremes might well be compensated for by the findings in a representative sample if such were to be had. And again, absolute production on the part of various social classes means little by itself. The demands of any particular society may absorb alarming differences in the production of its various social classes, to say nothing of the interchange that takes place between classes. If the supply of lower classes fails to meet the demand for those classes, then it is idle to view with alarm any production rate in those classes. It is only when the supply is in danger of exceeding the demand that an inventory of stock becomes really pertinent, and even then it would seem wise to direct a fair amount of attention to the reasons for the demand as well as enquiring into the sources of supply. The writer does not have access to the literature on this subject, which may not be entirely a disadvantage, and so exact references will have to be omitted.

The statistics of Japan, fine as they are, give no help on the problem of differential fertility. For a clear exposition of the problem it would be necessary to have complete data on deaths and on births, size of family and especially completed families, by age of father and mother and duration of marriage, divided into groups differentiated by some real difference, a difference that has some biologic significance such as economic status or amount of physical energy expended in gaining a livelihood. Either nothing at all, or at best only a makeshift analysis can be made of data classified entirely under rubrics of arbitrary definition, that is where all workers engaged in industry are classified as such regardless of whether they are clerks, workers in mills or presidents of corporations. Clearly there is an enormous range in the social status of such people, and to put them into one group makes it impossible to discover if any difference exists in their biologic activity. This becomes strikingly true if we consider such a group as that classified as unoccupied, wherein we find retired persons living on their incomes and persons in jails, asylums and other institutions. This is mixing biologic black with biologic white under the heading of rose and gold.

THE POPULATION OF SENDAI

In an effort to throw a faint light on the problem in Japan it was decided to study certain manifestations of fertility as exhibited by the certificates of registration of births. For convenience the city of Sendai was chosen. The city falls in the class of over a hundred thousand since in 1920 it had a population of 118,984. It is located in the north-eastern part of the main island, or Tohoku District, in Miyagi prefecture close to the coast. It serves a large agricultural area and so it contains a large percentage of persons engaged in the business of selling supplies to this area. Though no large industries of major importance are located in Sendai, there are enough of the smaller ones so that people engaged in industry lead in numbers, with those of commerce coming next. The general character of the population of the city is indicated in Table 1, which shows that the city

TABLE 1

Showing the population of Sendai, Japan in 1920 by occupation and the percentage distribution of the same. The figures under each heading include all members of the household

OCCUPATION	NUMBER	PER CENT
Industrial	32,468	27.29
Commercial	29,767	25.03
Officials and professions	23,180	19.48
Unoccupied, income, etc	13,213	11.10
Communications	11,368	9.56
Others, day laborers, etc	4,133	3.47
Agriculture	3,974	3.33
Mining	642	.54
Servants	142	.12
Marine, fishing, salt, etc	97	.08
Total	118,984	100.00

is essentially one of commerce and industry. There is a relatively small number listed under agriculture, because such workers live on farms and are not apt to be city dwellers. There is little mining or quarrying in the neighborhood and still less fishing or other marine industries. Such activities are carried on by persons who live in a nearby town on the coast.

And here the occupational groups may be defined, since the same classification will be maintained throughout this article.

1. Under *Industry* comes cement making, ceramics, metal manufactured products, production of machinery, chemical industry, textiles, paper, leather and horn products, wooden articles, edibles, wholesale tailoring, house building, painters and decorators, printing and the manufacture of printing machinery, manufacture of small objects such as pens, instruments, etc., development of natural power and other industrial pursuits not mentioned above.

2. *Commercial* includes salesmen, brokers, bankers, insurance men, warehousing, hotels, lunchrooms and others not listed.

3. *Officials*, military and naval, court officials, religious workers, teachers, museum and library workers, physicians, writers, newspapermen and the like.

4. *Unoccupied* includes persons living on income from property, pensions, etc., jail and asylum inmates, students, etc.

5. *Communications*, mail, telephone, telegraph and all forms of public transportation.

6. *Others*, day laborers and all types of intermittent workers for hire.

7. *Agriculture*, farming, sericulture, animal husbandry, forestry, etc.

8. *Mining*, extraction of minerals and quarrying.

9. Those who live in their own homes and who go out to serve. This qualification accounts for the small number found in the city under this classification.

10. *Marine*, fishing, salt making, shell and seaweed collection, etc.

From the above it is clear that the groups from a biologic standpoint are not mutually exclusive for any one variable. If we take, for instance, income, since it is directly related to the character of the environment that a man surrounds himself with, we find that almost any group contains a mixture of incomes that vary through a wide range. If on the other hand we take expenditure of energy in the form of physical work we find the same thing to be true. The group classified under *Communications* contains not only the men who are engaged in heavy physical labor in the maintenance of the right-of-way but it also contains the ticket seller who sits on a stool dispensing tickets, and although ticket sellers might contend that they are a hard working lot, obviously they do not compare with the track men on the basis of physical work. Outside of the fact that arbitrarily these people were placed in the same group in a census table, they have nothing in common, either socially, economically or biologically. And so the classification is practically useless for our purpose, but since it is the only one available we must make the best of it.

THE DISTRIBUTION OF BIRTHS

Through the courtesy of the Registrar in Sendai certificates of birth were abstracted for one year. The year 1920 was chosen because the National Census of that year made available census counts of persons by age, marital state and occupation, classified as has been

described above. Only one year was taken because the labor of abstracting the records was so great as to prohibit the inclusion of more than one year. To insure accuracy in registration only legitimate births were taken and only those births occurring to fathers who lived and work in Sendai in 1920. This gave a sample of 3,154 legitimate births divided into ten groups by occupation of the father. Since this sample applies to one year only it is somewhat defective. It represents only those births that occurred among fathers of specified occupations in the year 1920, and obviously such births are subject to variation through chance alone. Only one fisherman was a father in 1920, whereas in 1919 there might have been five or ten fishermen represented among the fathers and in 1921, another number. Or there might have been none in 1919 and 1921. Such small entries would be subject to a variability relatively wider than the values based on occupations having larger numbers, and in this article no importance or significance will be attached to entries of the former type.

From the certificate of registration of birth was taken the age of father, occupation of father, month of birth, month of registration and sex of child. This was all the information available. The certificate gives nothing on the size of family, duration of marriage or birth order of child except in the last named as applying to the sex of the child registered. That is, if the child registered is a male the certificate will show whether it is the first, second or third male, but it will not indicate how many females have been born to the same mother nor the order in which males and females fall. Since it was desired to know if the sample of births taken from Sendai resembled one taken from all Japan the percentage distribution of births by months was calculated for all Japan and for Sendai in 1920. The latter were further divided and the percentage distribution by months calculated for births occurring among fathers engaged in industry, commerce, and official work and the professions, since these three groups are the ones that show the largest frequencies. These data are assembled in Table 2.

Examination of the table and graph shows that in spite of using only one year as a sample, the distribution of the births for Sendai is very close to that of all Japan and the distributions for the occupations chosen are sufficiently close to justify certain tentative conclusions, since the inference is that if the percentage distributions of the various samples do not differ widely from a distribution for all Japan, then their numbers are large enough to insure a fair amount of stability.

TABLE 2

Showing the percentage distribution by months for births in all Japan and in Sendai, Japan, for the year 1920, *and the percentage distribution by months for the occupations as indicated. The births in all Japan for* 1920 *numbered* 2,025,564 *and in Sendai,* 3,154

MONTH	PER CENT OF BIRTHS: ALL JAPAN	PER CENT OF BIRTHS: SENDAI	SENDAI, 1920		
			Per cent. born to fathers in industry.	Per cent. born to fathers in commerce	Per cent. born to fathers in professions.
January	12.5	13.8	13.3	14.0	14.4
February	10.2	9.8	9.2	9.6	10.9
March	12.8	12.8	13.3	12.0	11.2
April	6.7	6.3	5.7	7.3	5.4
May	6.8	6.8	6.7	6.2	7.1
June	6.2	7.3	7.0	7.5	7.1
July	6.9	6.8	5.9	8.3	6.6
August	7.6	7.8	6.8	8.2	8.4
September	8.7	8.0	9.6	7.5	8.4
October	7.2	7.6	8.0	6.5	9.3
November	7.9	8.0	8.9	7.4	7.1
December	6.4	5.2	5.7	5.7	4.2

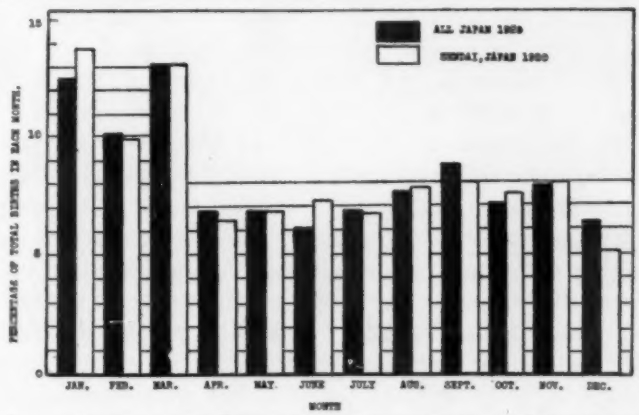

FIG. 1. GRAPH SHOWING THE PERCENTAGE DISTRIBUTION BY MONTHS FOR BIRTHS IN ALL JAPAN AND IN SENDAI, JAPAN, FOR THE YEAR 1920

The absolute frequencies upon which were based the percentages for Sendai together with frequencies for other classes of occupations not mentioned in Table 2 are show in Table 3.

TABLE 3

Showing the number of legitimate births in Sendai, Japan, in 1920 by month and occupation of father as indicated

OCCUPATION	MONTH												
	Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	Sept.	Oct.	Nov.	Dec.	Tot.
Agriculture	14	15	13	9	12	9	7	14	8	10	17	8	136
Marine	...	...	...	...	...	...	...	1	...	...	...	...	1
Mining	1	...	2	1	...	2	2	1	1	...	...	...	10
Industry	103	71	103	44	52	54	46	53	74	62	69	44	775
Commerce	139	95	119	72	61	74	82	81	74	64	73	56	990
Communication	23	29	34	12	17	21	12	25	16	18	15	15	237
Offic. & Prof	96	73	75	36	47	47	44	56	56	62	47	28	667
Others	22	11	21	10	16	12	8	7	9	15	13	9	153
Servants	...	...	...	...	...	...	...	...	...	...	1	...	1
Unoccupied	36	15	31	14	10	12	14	7	13	10	16	6	184
Total	434	309	398	198	215	231	215	245	251	241	251	166	3154

Examination of Table 3 indicates at once that the frequencies for certain occupations are so low that they would be subject to a wide variability through the operation of chance. The occupations showing low absolute frequencies are marine, mining and servants, and because the experience is so small in these cases they will be excluded from discussion. The remaining seven classes of occupations exhibit frequencies sufficiently high and their distributions by months are close enough to the total either for Sendai or for all Japan to justify one to a reasonable extent in comparing findings in one occupation with those in another. As would be expected, the smaller the frequency the greater is the tendency for it to deviate from the distribution for Sendai or all Japan. In the case of the frequencies for industry, commerce and the professions, as has been noted previously the deviation is slight. With respect to the other occupations, if their frequencies be distributed by months on a percentage basis, the resulting curve may deviate in level at its respective points, but the general shape of the curve very much resembles a similar curve based on values for all Japan. Proceeding on the assumption that births in Sendai for one year in the case of certain occupations represents a fairly satisfactory sample, they were classified according to the age of father and occupation. The figures for this tabulation are shown in Table 4.

There is nothing of particular note exhibited by the table. In all occupations having relatively high frequencies the distributions are fairly symmetrical. The largest number of births for such frequencies usually falls in the age group 30–34 or 35–39. The exceptions to this

TABLE 4

Showing the number of legitimate births in Sendai, Japan, in 1920 *by occupation and age of father*

OCCUPATION	AGE										
	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–	Total
Agriculture	..	7	21	28	29	31	14	5	1	..	136
Marine	..	..	...	...	1	...	...	...	..	..	1
Mining	..	1	4	2	...	2	1	...	..	..	10
Industry	1	26	155	190	171	135	66	22	4	3	773
Commerce	1	21	171	248	233	162	99	40	9	3	987
Communication	..	3	42	53	55	43	29	8	4	..	237
Offic. & Prof	..	6	66	185	211	114	53	20	7	..	662
Others	..	3	10	28	32	39	26	8	5	..	151
Servants	..	..	...	...	...	...	...	1	...	...	1
Unoccupied	1	3	40	31	38	31	22	12	4	2	184
Total	3	70	509	765	770	557	310	116	34	8	3,142

rule are the occupations *Agricultural* and *Others*, both of which have relatively low total frequencies which show the highest frequency in the age group 40–44. There is, of course, no significance in this deviation.

AGE SPECIFIC BIRTH RATES

The next step is to transfer the number of births from absolute frequencies to a rate basis wherein the number of married males in each occupational group is considered. The numbers of such married males by age and occupation have been taken from the *Results of the First National Census* and are shown in Table 5.

TABLE 5

Showing the number of married males in Sendai, Japan, in 1920 *by age and occupation as indicated*

OCCUPATION	AGE										
	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60 & up	Total
Agriculture	5	51	107	126	109	142	98	111	87	119	955
Marine	1	1	2	8	1	4	2	2	.....	2	23
Mining	..	8	16	17	18	27	22	11	11	3	133
Industry	12	271	914	1,016	916	801	552	526	342	400	5,750
Commerce	7	165	566	834	835	815	681	545	391	502	5,341
Communication	4	86	365	462	428	349	238	168	90	64	2,254
Offic. & Prof	5	892	387	616	540	447	348	332	193	221	3,981
Others	2	14	62	83	117	136	110	99	76	101	800
Servants	..	.....	4	6	5	4	1	1	1	.....	22
Unoccupied	3	39	87	106	117	115	101	138	150	436	1,292
Total	39	1,527	2,510	3,274	3,086	2,840	2,153	1,933	1,341	1,848	20,551

The frequencies of married males in Table 5 are fairly well correlated with the frequencies of births as shown in Table 4, that is, in general an occupation or age group that shows a relatively high frequency in Table 5 shows a relatively high frequency in the corresponding cell of Table 4 and the converse is equally true.

Using the values for the number of births as given in Table 4 and the number of married males by age and occupation as shown in Table 5, rates have been calculated which express the number of births per hundred married males by age and occupation. These rates may be thought of as paternity rates made specific for age, occupation and marital state. They are shown in Table 6.

TABLE 6

Showing the number of births per hundred married males in Sendai, Japan, in 1920 *by age and the occupations as indicated. Rates shown in the inner square are based upon frequencies large enough to make them fairly stable*

OCCUPATION	AGE										TOTAL RATE
	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–	
Marine	0	0	0	0	100.0	0	0	0	0	0	4.4
Mining	0	12.5	25.0	11.8	0.0	7.4	4.6	0	0	0	7.5
Agriculture	0	13.7	19.6	22.2	26.6	21.8	14.3	4.5	1.2	0	14.2
Industry	8.3	9.6	17.0	18.7	18.7	16.9	12.0	4.2	1.2	.8	13.5
Commerce	14.3	12.7	30.2	29.7	27.9	19.9	14.5	7.3	2.3	.6	18.5
Communication	0	3.5	11.5	11.5	12.9	12.3	12.2	4.8	4.4	0	10.5
Offic. & Prof	0	0.7	17.1	30.0	39.1	25.5	15.2	6.0	3.6	0	16.8
Unoccupied	33.3	7.7	46.0	29.2	32.5	27.0	21.8	8.7	2.7	.5	14.2
Others	0	21.4	16.1	33.7	27.4	28.7	23.6	8.1	6.6	0	19.1
Servants	0	0	0	0	0	0	0	100.0	0	0	4.6

A point to be noted in connection with the table is that while the rates for different occupations differ somewhat there is a relative lack of variability between the rates within each occupational group between the ages 25 and 45. That is, among fathers who were engaged in industry, the number of births per hundred married males is essentially the same for fathers at 27.5 years of age and 32.5, 37.5 and 42.5 years. And the same is more or less true for the other occupations except officials, who show a pair of decidedly high rates for the ages 32.5 and 37.5 years. This may be an expression of a delayed age at marriage in this class which forces the reproductive activity into an older age group. There is also a high rate attributed to the unoccupied class in the age group 25–29, but although this rate is included in the inner square of Table 6, it is based on a low frequency and is therefore subject to wide chance fluctuations.

The total rates for seven occupations: namely, communications, industrial, unoccupied, agriculture, officials and professions, commerce and miscellaneous, have been plotted and are shown in Figure 2. A glance at Figure 2 shows that the largest (19.1) number of births per hundred married males occurred in the miscellaneous group. The remaining occupational groups are arrayed in descending order and have values as follows: commercial, 18.5; officials and professions, 16.8; agriculture, 14.2; industrial, 13.5; and communications, 10.5.

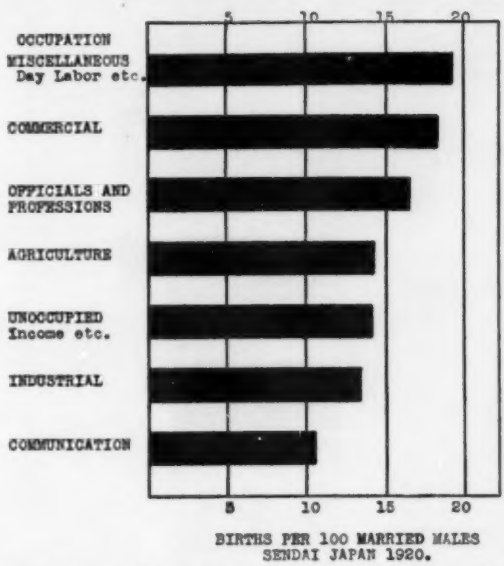

FIG. 2. DIAGRAM SHOWING BIRTHS PER HUNDRED MARRIED MALES IN SENDAI, JAPAN, IN 1920 FOR THE OCCUPATIONS AS INDICATED

Thus it is seen that the highest rate is about twice as great as the lowest rate, while the other five groups have their rates falling within a range of roughly three points, 13.5 to 16.5. The above rates apply only to those married males who became fathers in 1920 and as a result are not indicative of differential fertility between occupations except for that single year.

Real differential fertility can best be exhibited by the *completed* family since this represents the reproductive activity of the male, not for a single year, but the summation of his activities on a time base. Unfortunately, as has been noted previously, the data for completed families are not available. However an approach to the

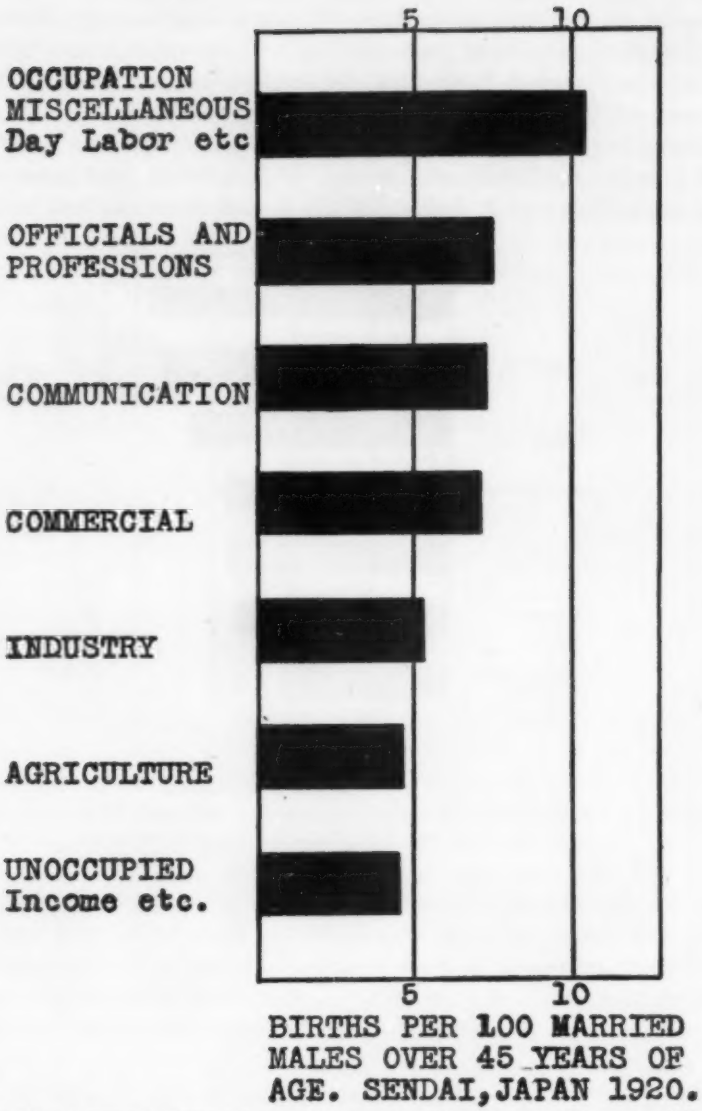

FIG. 3. DIAGRAM SHOWING THE NUMBER OF BIRTHS PER HUNDRED MARRIED MALES IN SENDAI, JAPAN, IN 1920, OCCURRING AMONG FATHERS WHO WERE 45 YEARS OF AGE OR OVER, BY THE OCCUPATIONS AS INDICATED

question can be made by considering only those births that occur to fathers of the older ages. That is, if we take out of the total population, married males over forty-five years of age, and out of the births only those that occurred to fathers who were forty-five years of age or over, the two will result in a paternity rate applying to fathers of the ages forty-five years and over. This is done on the assumption that a birth occurring to a father who is more than forty-five years of age represents a case of more than average fertility and that if the facts were known, the size of completed families by occupation would be positively correlated with the number of males in each occupational group who became fathers after they had reached forty-five years of age. The correlation would not necessarily be perfect, but at least it is probable that it would be positive and fairly high. The age forty-five was chosen as a lower limit because it was felt that the correlation would be higher in such a case than when the lower limit was allowed to drop into younger age groups. These rates have been computed and are shown in Figure 3.

The diagram shows a slightly different order than is exhibited by the rates shown in Figure 2. Those fathers having miscellaneous occupations (day laborers, etc.) still come at the top of the list with a rate of 10.6, followed by officials, communications and commerce, the latter three showing essentially the same rates, namely, 7.7, 7.3, and 7.2 respectively. The last three groups are industry, 5.3, and agriculture and unoccupied, both showing the same rate of 4.8. That these rates do contain an element of differential fertility is suggested by the fact that the order of the rates is not related to the order of occupations when these are arrayed with reference to the number of married males over 45 contained in the respective groups. For instance commerce comes first in order with the largest number of married males over 45 (2,119) but is fourth on the basis of births per hundred married males over 45. Industry is second in numbers (1,820) but is fifth in births. Officials are third in numbers (1,094) and second in births; unoccupied, fourth in numbers (825) and seventh in births; communications, fifth in numbers (560) and third in births; agriculture is sixth in numbers (415) and sixth in births; while the most striking difference is shown by the miscellaneous group, which is last in numbers (386) but first in births.

DISCUSSION

The data presented in this paper apply only to a single year, 1920, and this should be borne in mind when any inferences are drawn.

Thus the rates based on fathers of all ages may be expected to vary somewhat from the findings for any other single year, and one is not justified in concluding that real differential fertility is related to occupation as would be shown by the arrangement of rates based on fathers of all ages. On the other hand if it is granted that births occurring to fathers who are forty-five years of age or over, represent the activities of males that are more than usually fertile and that completed families sired by these males will probably be larger that those sired by males who did not become fathers as late as forty-five years of age, then it may be supposed that differential fertility does exist between different occupational groups, and that these groups bear the same relation to each other with respect to fertility or possibly the size of completed family, as do the number of births per hundred married males over forty-five by occupation (Figure 3). On this basis, it is seen that in Sendai, Japan, in 1920, those males having miscellaneous occupations, such as day laborers, etc., lead in the number of births that they furnish. Next come the officials and professions, communications and commercial groups, all with about the same fertility, roughly about seven births per hundred married males over forty-five. The last three groups of industry, agriculture and unoccupied have rates around five or about half the rate of the miscellaneous class. Reference to Table 1 shows the constitution of the city in 1920 and from this it is seen that if one is to judge from the differential fertility as shown by the rates of Figure 3 the constitution of the city is in the process of being altered. The two leading groups on the basis of numbers are industry and commerce, but when we turn to the paternity rates we find them well down the list. On the other hand, day laborers and others of miscellaneous occupations comprise only a little more than three per cent of the population, yet they lead in births both in rates based on all ages and rates for forty-five and over. In short, a class that is the lowest in point of numbers is the largest contributor of births on a rate basis, and at the same time, if the year 1920 is taken as typical of a general state of affairs, the industrial and commercial occupational classes, strongest in numbers, are reproducing in such a way that they may not maintain their relative positions in the city population. Some earnest and well-meaning people may become greatly alarmed over this prospect but the writer does not feel impelled to join in the uneasiness until further facts are known. It is possible that there is an ideal distribution of the population wherein the different occupational groups bear such a relation to each other

that their combined activities result in the utopian harmony so much to be desired, but such a society would be more or less fixed both in its activities and its composition. In the introduction to this paper it was pointed out that social classes—and this is virtually synonymous with occupational classes—are not fixed but are in a constant state of flux, and further, this movement is in all probability, predominantly in one direction. That is, while there is some tendency for persons in higher classes to be displaced downwards, the vast majority of movements take place from lower classes to higher ones. Not only is there this inter-class movement but the character of society itself changes, and this change for any given area is probably determined by economic forces, at least these must be of major importance. It seems quite clear that a society engaged in the main in agriculture would have a different social constitution from one that devotes itself mostly to industry and between these two states may be any number of situations made up of varying proportions of the two, and no one of them will be fixed. And so when a census is taken of the various social or occupational groups at any one time, there is no reason for supposing that the proportions shown by such a census should remain fixed and be repeated at a later date. The fact that Sendai has 3.47 per cent engaged in miscellaneous occupations in 1920 should cause no lamentation in 1925 if the percentage has risen to 4.47 unless it can be shown that there is no demand or place for the added one per cent. Without knowing the real facts with reference to demand for the different occupational groups in Sendai, observation would lead one to conclude that the differential fertility as indicated by rates for fathers over forty-five years of age is just about what Sendai needs. This must not be construed as a suggestion that there is some mysterious connection between economic demand and reproductive machinery so that when there is a marked demand for laborers there is a high birth rate among laborers. There may or may not be such a relationship among human beings. However the writer merely wishes to emphasize the point that change in the relative proportions of social or occupational classes is not unexpected and such a change, no matter what direction it may take nor what magnitude it may reach, is not alarming nor to be decried until it can be shown that it is not consistent with the trend of the social organization as a whole. Of course it may be contended that marked differential fertility may *determine* the trend of society, but it is doubtful whether it has ever done so in the past, and it would seem that the burden of proof rests with the one who makes such a contention.

In the introduction to this paper a brief description was given of the general character of the city of Sendai as shown by Table 1. In addition it may be noted that the city is and has been the administrative center of a prefecture and also contains an army post, both of which facts help to explain the proportion of officials. One of the Imperial Universities with its several faculties is also included within the city limits, accounting for the relatively high proportion of professional men. The year 1920 fell within a period of rapid material growth with considerable building going on, possibly the expression of the prosperity that immediately followed the war, and at such a time there is nothing unusual in a high proportion of day laborers. In 1928 all of the above facts still remain, with the possible exception of building, which may not be so active as it was in 1920. And so it may be inferred that the trend of society in Sendai in 1920 was very much in the direction that would be aided rather than hindered by the apparent differential fertility that is suggested by the facts set forth in this paper. Sendai, in common with all Japan, is in the transitional stage where society is changing from an exclusively agricultural one, to one that contains an increasing proportion of industry, and in Japan this transition is taking place at a rate that probably exceeds the rate of change in any other country. With this fact in mind it would seem that the differential fertility exhibited, fairly well fits the present trend of society. At least it would require a considerable amount of data, both economic and sociologic, to conclude that it did not.

CONCLUSIONS

1. An analysis is presented of the births in Sendai, Japan, for 1920 by occupation and age of the father.

2. When placed on a rate basis by occupation and including fathers of all ages, the rates array themselves in descending order as follows: miscellaneous, commercial, officials and professions, agriculture, unoccupied, industrial and communications.

3. If the rate is made specific for age and includes only those fathers forty-five years of age and over, the rates by occupations array themselves in descending order as follows: miscellaneous, officials and professions, communications, commercial, industry, agriculture and unoccupied.

EVIDENCE OF A SEASONAL CYCLE IN HUMAN GROWTH[1]

BY JOSEPH BERKSON

In a study previously published by the present writer (1) dealing with a problem quite different from the one considered here, data on the height and weight of children between birth and six years of age were utilized. These data were obtained from the results of R. M. Woodbury issued as a report from the Children's Bureau of the United States Department of Labor under the title *Statures and Weights of Children under Six Years of Age* (2). They give, among other things, for each month of age between one and seventy-two months, for males and females separately, the mean height and mean weight of groups of children of specified ages. Each of these means was obtained from a large number of observations, the average number being about 15,000. When they are plotted against age a continuous smooth curve is obtained, reflecting the homogeneity of the series as a whole and the reliability of the individual means. Figure 1 shows this graphically for height and weight and the two sexes separately.

APPEARANCE OF A YEARLY CYCLE

Presumably the trend of these points on age as shown in Figure 1 is a representation of the growth of the individual over the first seventy-two months of life. Certain considerations must be weighed which may modify this interpretation, and some of them will be taken up in a later part of the paper. But for the purposes of the immediate argument we may adopt the usual view that Figure 1 gives a picture of the way the individual grows. Taking a broad view of them it is seen that for both height and weight the points have a parabolic trend on age with the convexity of the parabola facing upward. But if one examines the succession of points more minutely one gets the impression of another character superimposed on the first in each of the trends considered. It consists of a wave form sweeping up and down over the general trend. A further examination will show that

[1] From the Institute for Biological Research of the Johns Hopkins University.

the period of the wave appears to be about one year, the crest of the first half appearing below the central trend, the crest of the second half above. Now if one finds in a growth curve a wave change of yearly periodicity, one thinks immediately of a seasonal effect during the growth process. This suggestion from the appearance of Figure 1 is the point of departure for the analysis that follows.

We say then, that on the evidence of graphical appearance the observations considered here seem to contain a wave of one year period. But judgment from graphs is largely a matter of aesthetics. What will seem quite convincing to one observer will appeal as not at all so to another, and between the two any decision is arbitrary. The task at hand is to obtain some objective measure of whether what appears in the data as a regular cyclic change is really one. I have attempted to get at such a measure by way of the following reasoning.

IS THE APPARENT CYCLE SIGNIFICANT?[1]

The concept of a seasonal cyclic change in growth involves the idea, first, that there is a normal process of growth, more or less fixed in its basic characters according to which an individual increases in stature and weight as age advances. Were no influences at work other than those implied in the biologic process of ageing itself, growth would tend to take place along some central course, definable as a smooth curve against age. Of course no particular individual would be expected to follow this curve exactly; there would be variability as regards this for individuals just as there is variability for other biologic characters. However, we would expect that the deviations of individuals from the central curve of growth would have the character of *random* deviations. Indeed, the growth curve for any species under given fixed conditions might well be defined as the growth curve about which individuals on the species vary at random. If now, on the foundation of a growth process basically so defined, special environmental forces have play, the effect on the observations will be to detract from them, to some degree, their random character and to introduce into their deviations from the central tendency an element of system. The exact nature of the systematic character will, of course, depend on the exact nature of the influences which give rise to it. In the specific instance where an effect due to season was operative, the deviations would be correlated with the time of the year. Seen in the light of this analysis our question may be stated in the following terms. "Do the observed means, in their deviations from a

central tendency, depart from random character, and if so, is it in the particular respect that the deviations of one part of the year differ significantly from those of another part of the year?" Put this way, we may hope to give the question an exact quantitative answer.

Obviously if we are to study deviations from a smooth central norm, we will have to be able to evaluate the position of this norm for every age at which we have an observation. Probably the most precise estimates would be obtained by fitting an appropriate curve functionally to the series of observations as a whole and taking for the central norms the theoretical values derived from this function. But the choice of an "appropriate curve" is a nice matter which requires a great deal of systematic statistical labor. For the purposes in hand we may use more approximate methods which are, practically, a great deal simpler. The one used for this analysis was the following. To the monthly means of each two successive years, i.e., the first and second year, second and third year, etc., in the series, a parabola of the second order was fitted by least squares. This gave five parabolas from which theoretical values for the means were obtained. For each month of the second, third, fourth and fifth years there were two values, one from each of the overlapping parabolas. For those years the mean of the two predicted values was used as an estimate of the norm. For the first and sixth year the appropriate individual value obtained was used.

In the following will be presented in rather great detail an analysis of the observations in the male-stature series, and after that a summary statement of the results of a similar treatment of the three other series, male-weight, female-stature, and female-weight.

TESTS OF SIGNIFICANCE FOR MALE STATURE

To be concrete there is listed in Table 1 the theoretical estimate for the mean height at each month of age, and the deviation from it of the observed mean.

Suppose now that each monthly mean deviated from the estimated central value only from random influences. It would follow that each mean would be as likely to fall on either side of the norm, and by any given amount, as the next succeeding month. On the other hand, if the observations contained a yearly wave of the form hypothesized, i.e., the first half having its crest below, and the second half its crest above the norm, then in the first six months of each year the deviations would be likely to fall below and in the second six months above

the calculated central value. Further, the amount of the deviation would tend to progress from month to month. How do the deviations actually fall? To ascertain this, there was determined the mean of the deviations for the first months of the six recorded years, the mean of the second months, the third months, etc., and so for all twelve months. The numerical values are given in Table 1, B, and shown graphically about a central zero line in Figure 2, A. The figure shows plainly that the deviations do indeed have a progressive character and precisely of a kind that would corroborate the original idea of the presence of a wave in the observations.

We are still, however, in the realm of pictorial impression. What we wish, for scientific conviction, is a *numerical* statement of what the situation is. For the purpose we may apply to the results of Table 1 a number of statistical "tests of independence." The general procedure will be as follows. Each year will be divided into a first half consisting of the first six recorded age months, and a second half consisting of the later six months. We will inquire then, concerning the deviations in the respective halves, "What is the probability that the first half is significantly different from the second half?" Before proceeding to the actual calculations it is desirable to recall

TABLE 1

Male, Stature

A. Central Values from Fitted Parabolas

MONTH OF YEAR	YEAR OF AGE													
	First	Second			Third			Fourth			Fifth			Sixth
	Value Parab. 1 *in.*	Value Parab. 1 *in.*	Value Parab. 2 *in.*	Average *in.*	Value Parab. 2 *in.*	Value Parab. 3 *in.*	Average *in.*	Value Parab. 3 *in.*	Value Parab. 4 *in.*	Average *in.*	Value Parab. 4 *in.*	Value Parab. 5 *in.*	Average *in.*	Value Parab. 5 *in.*
1	21.99	29.51	29.37	29.44	33.59	33.61	33.60	36.70	36.67	36.69	39.33	39.24	39.29	41.66
2	22.76	29.96	29.77	29.87	33.89	33.88	33.89	36.94	36.91	36.93	39.54	39.45	39.50	41.85
3	23.51	30.39	30.15	30.27	34.18	34.15	34.17	37.18	37.14	37.16	39.74	39.65	39.70	42.05
4	24.24	30.78	30.53	30.66	34.46	34.41	34.44	37.41	37.37	37.39	39.93	39.86	39.90	42.25
5	24.93	31.15	30.90	31.03	34.74	34.68	34.71	37.64	37.60	37.62	40.13	40.06	40.10	42.44
6	25.60	31.49	31.26	31.38	35.00	34.94	34.97	37.88	37.83	37.86	40.32	40.26	40.29	42.63
7	26.24	31.81	31.62	31.72	35.26	35.20	35.23	38.10	38.05	38.08	40.50	40.46	40.48	42.83
8	26.85	32.09	31.97	32.03	35.52	35.45	35.49	38.23	38.28	38.31	40.69	40.66	40.68	43.02
9	27.44	32.35	32.31	32.33	35.76	35.71	35.74	38.55	38.49	38.52	40.87	40.86	40.87	43.21
10	28.00	32.59	32.64	32.62	36.00	35.96	35.98	38.77	38.71	38.74	41.05	41.06	41.06	43.40
11	28.53	32.79	32.96	32.88	36.22	36.21	36.22	38.99	38.92	38.96	41.22	41.26	41.24	43.59
12	29.04	32.97	33.28	33.13	36.44	36.45	36.45	39.21	39.13	39.17	41.40	41.46	41.43	43.78

TABLE 1.—(*Continued*)

Male, Stature

B. Deviations of Observed Means from Central Norm

MONTH OF YEAR	YEAR OF AGE												AVERAGE
	First		Second		Third		Fourth		Fifth		Sixth		
	Theoretical Mean	Observed Mean	Theoretical Mean	Observed Mean	Theoretical Mean	Observed Mean	Theoretical Mean	Observed Mean	Theoretical Mean	Observed Mean	Theoretical Mean	Observed Mean	
		Deviation Obs.-Theor.		Deviation Obs.-Theor.		Deviation Obs.-Theor.		Deviation Obs.-Theor.		Deviation Obs.-Theor.		Deviation Obs.-Theor.	
1	21.99	21.16 — .83	29.44	29.36 — .08	33.60	33.60 .00	36.69	36.67 — .02	39.29	39.21 — .08	41.06	41.00 — .06	— .18
2	22.76	22.47 — .29	29.87	29.73 — .14	33.89	33.90 + .01	36.93	36.90 — .03	39.50	39.51 + .01	41.85	41.96 + .11	— .06
3	23.51	23.58 + .07	30.27	30.15 — .12	34.17	34.08 — .09	37.16	37.09 — .07	39.70	39.69 — .01	42.05	42.10 + .05	— .03
4	24.24	24.53 + .29	30.66	30.42 — .24	34.44	34.35 — .09	37.39	37.31 — .08	39.90	39.81 — .09	42.25	42.05 — .20	— .07
5	24.93	25.35 + .42	31.03	30.89 — .14	34.71	34.55 — .16	37.62	37.54 — .08	40.10	39.91 — .19	42.44	42.34 — .10	— .04
6	25.60	26.08 + .48	31.38	31.26 — .12	34.97	34.95 — .02	37.86	37.83 — .03	40.29	40.25 — .04	42.63	42.59 — .04	+ .04
Average	.....	..... + .023	.....	..... — .140	.....	..... — .058	.....	..... — .052	.....	..... — .067	.....	..... — .040	— .0567
7	26.24	26.68 + .44	31.72	31.77 + .05	35.23	35.31 + .08	38.08	38.09 + .01	40.48	40.47 — .01	42.83	42.93 + .10	+ .11
8	26.85	27.21 + .36	32.03	32.12 + .09	35.49	35.60 + .11	38.31	38.38 + .07	40.68	40.76 + .08	43.02	43.22 + .20	+ .15
9	27.44	27.59 + .15	32.33	32.34 + .01	35.74	35.85 + .11	38.52	38.61 + .09	40.87	40.97 + .10	43.21	43.20 — .01	+ .08
10	28.00	28.02 + .02	32.62	32.64 + .02	35.98	36.03 + .05	38.74	38.87 + .13	41.06	41.21 + .15	43.40	43.32 — .08	+ .05
11	28.53	28.47 — .06	32.88	33.05 + .17	36.22	36.16 — .06	38.96	38.98 + .02	41.24	41.27 + .03	43.59	43.48 — .11	— .002
12	29.04	28.91 — .13	33.13	33.23 + .10	36.45	36.48 + .03	39.17	39.19 + .02	41.43	41.29 — .14	43.78	43.87 + .09	— .01
Average	.....	..... + .130	.....	..... + .073	.....	..... + .053	.....	..... + .057	.....	..... + .035	.....	..... + .022	+ .0638

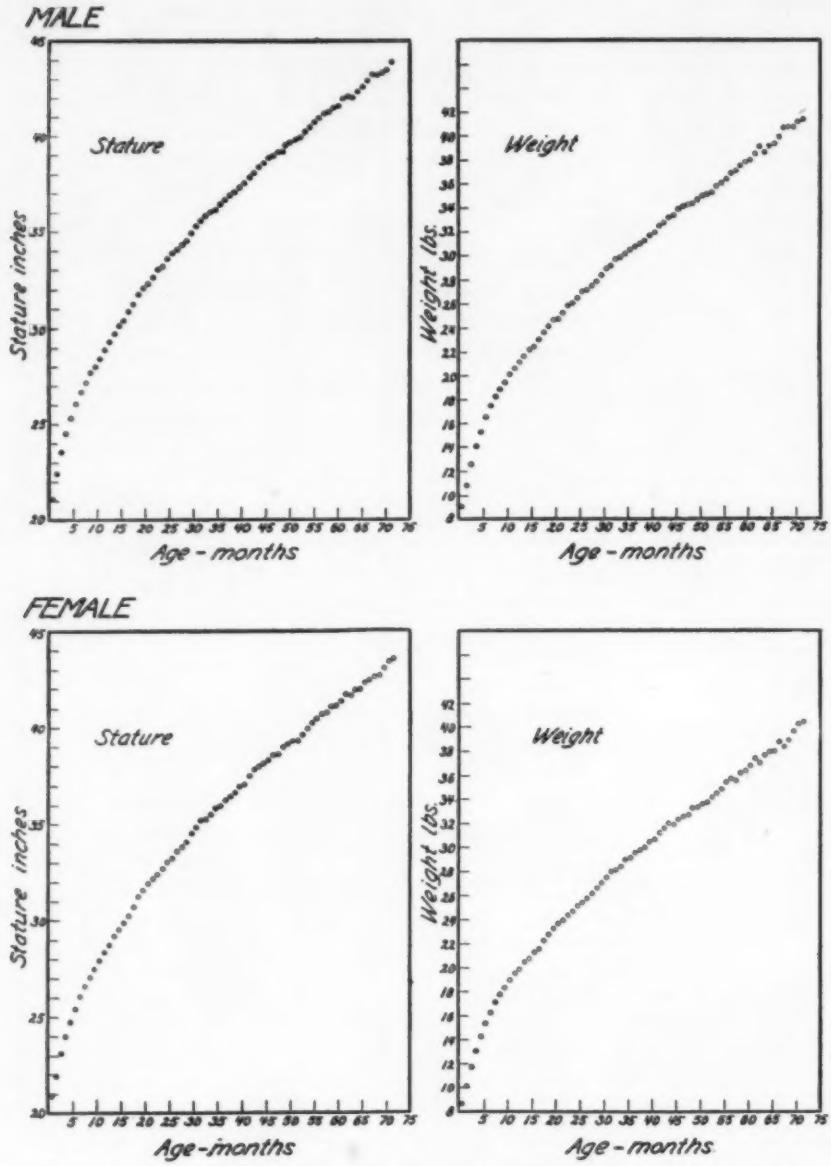

FIG. 1. MONTHLY MEANS VS. AGE—OBSERVED; FROM DATA OF WOODBURY.

a few of the elementary principles of statistical theory which will be needed. The basic statistic utilized for the estimation of probability is the standard deviation, symbolized by the Greek letter σ. The probability of any given departure x from an expected value, when random deviations only are involved, is evaluated from the ratio of x to σ. The greater the ratio, the smaller will be the probability that the departure has occurred fortuitously. It is the general practice to consider a value significantly determined within a range

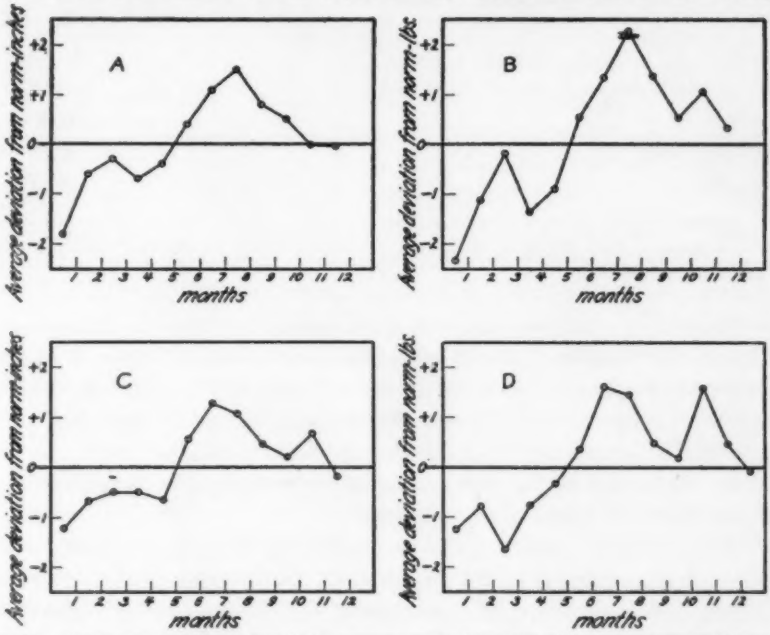

FIG. 2. AVERAGE OF MONTHLY DEVIATIONS FROM NORM VS. CORRESPONDING MONTH. A, MALE STATURE. B, MALE WEIGHT. C, FEMALE STATURE. D, FEMALE WEIGHT

of plus or minus twice its standard deviation. Therefore, if a difference between two quantities is as great as twice the standard deviation of the difference we may conclude that probably the quantities are in fact different. For the particular ratio two times the standard deviation the probability that the result is fortuitous is 0.046, that is, less than one in twenty. For larger values of the ratio the probability rapidly decreases; for three times the standard deviation it is 0.0026, for five times, less than one in a million.

We may now proceed to an estimation of the probability under several heads as follows.

A. Difference of the magnitude of the mean deviations in the first and second halves of the six growth years

If the first and second halves of the year are not differentiated during the growth process, the deviations from the norm in the one half should not differ significantly from the other. In Table 2 the facts as regards this are shown for the male-stature series.

TABLE 2

Male, Stature. Mean Deviation in First and Second Six Months

Average of Deviations, first six months	−0.0567
Average of Deviations, second six months	+0.0638
Difference	−0.1205
S.D. of Difference*	0.01390
Diff. ÷ S.D.	8.67

* The method of calculating the s.d. of each of these means and their difference is given in Appendix I.

From this result we say that a difference between the mean deviations of the first and second halves of the six years of growth studied, would have appeared as large as the one actually found, less than one time in a million, if the difference arose as a result of chance alone. We conclude therefore that the difference is *not* fortuitous.

B. Difference in the distribution of direction of the deviations in the first and second halves of the six years

Another test of whether the two halves of the year are differentiated in the growth process may be applied to the *direction* of the deviations. If they are equivalent, the number of deviations that fall above and below the norm should be the same for either half. Table 3 gives the facts as regards the distribution in the series being discussed here.

From the results we say, that were the two halves of each of the six years equivalent during growth, a difference in the proportion of deviations falling above the norm as great as the one actually found, would have occurred only once in a million times from chance alone. We conclude therefore that the difference is significant.

C. Distribution of individuals above and below the norm as between the two halves of the year, in each year separately

Table 3 of the original publication of Woodbury (2) gives for each month of the six recorded years of age, the actual distribution of

TABLE 3

Male, Stature. Signs of Deviations in First and Second Six Months

	Over Norm	*Under Norm*	*Total*
First Six Months	8	28	36
Second Six Months	28	8	36
Total	36	36	72

Proportion over norm F.S.M	0.222
Proportion over norm S.S.M	0.778
Difference	−0.556
S.D. of Difference*	0.1180
Difference ÷ S.D. of Difference	4.71

* See Appendix II.

heights of individuals the means for which we have been discussing up to this point. We may utilize these for a test similar to that given in *B*, using instead of the means for monthly groups, the individual heights from which these means were obtained. Since the number of individuals is large, we are able, in this instance, to apply the test separately to each year. The results are shown in Table 4.

Taking the customary minimum of twice the standard deviation as a criterion of probable significance we see that for four years out of six the results show a statistically significant difference in the two halves of the year conforming with our hypothesis. It need hardly be pointed out that where no statistically significant difference is found it does not mean that there is no difference; it means that the difference is not convincingly evident in that particular sample.

D. Distribution of individuals above and below the norm, as between the first and second halves of the year, for the six years taken together

We may now combine the results given in C for the corresponding halves of the six recorded years, and repeat the test for the experience as a whole. The results appear in Table 5.

We see here finally that taking the male stature series as a whole, it is overwhelmingly improbable that the individuals in the two halves of the six years should have distributed themselves so unequally about the norm as a matter of chance.

An analysis precisely parallel to the one presented for the male-stature series was made for each of the other series contained in the Woodbury report: male-weight, female-stature, female-weight. The details need not be reproduced here, since the results are exactly

TABLE 4

Male, Stature. Distribution of Individuals About Norm

	YEAR																	
	First			Second			Third			Fourth			Fifth			Sixth		
	Over norm	Under norm	Total	Over norm	Under norm	Total	Over norm	Under norm	Total	Over norm	Under norm	Total	Over norm	Under norm	Total	Over norm	Under norm	Total
First Six Months.....	5129	4087	9216	3678	4364	8042	3713	3996	7709	3492	3645	7137	3314	3402	6716	1602	1630	3232
Second Six Months...	5701	5320	11021	3769	3723	7492	3742	3716	7458	3654	3478	7132	3277	2977	6254	1234	1141	2375
Total.....	10830	9407	20237	7447	8087	15534	7455	7712	15167	7146	7123	14269	6591	6379	12970	2836	2771	5607
Prop.>Norm. F.S.M.....	0.5565			0.4573			0.4816			0.4893			0.4934			0.4957		
Prop.>Norm. S.S.M.....	0.5137			0.5031			0.5017			0.5123			0.5240			0.5196		
Difference.....	0.0392			−0.0458			−0.0201			−0.0230			−0.0306			−0.0239		
S.D. of Diff.....	0.007037			0.008022			0.008118			0.008350			0.008798			0.01360		
Diff. ÷ S.D.....	5.57			5.71			2.48			2.75			3.48			1.76		

TABLE 5

Male, Stature. Distribution of Individuals About Norm—Complete Experience

	OVER NORM	UNDER NORM	TOTAL
First six months	20928	21124	42052
Second six months	21377	20355	41732
Total	42305	41479	83784

Prop.> Norm F.S.M	0.497
Prop.> Norm S.S.M	0.512
Diff	−0.015
S.D. Diff	0.003454
Diff. ÷ S.D.	4.21

similar to those found for the first series. The definitive facts are shown in Figure 2B, C, and D, and in Table 6 which in summarized form is identical with Table 5 given for the male-stature series. The figure strongly suggests a cycle, and the table demonstrates numerically that for each series separately considered, the two halves of the year are differentiated in the data.

TABLE 6

Distribution of Individuals about Mean, Complete Experience

	SERIES		
	Male Weight	Female Stature	Female Weight
Proportion > Norm, First Six Months	0.481	0.494	0.4713
Proportion > Norm, Second Six Months	0.497	0.505	0.4908
Difference	−0.016	−0.011	−0.0195
S.D. of Difference	0.0035	0.00350	0.0035
Difference ÷ S.D.	4.6	3.2	5.6

DISCUSSION

It appears abundantly evident, taking all of what has been reviewed into consideration, that there appears in the data examined here a wave cycle of yearly periodicity, and this applies to the data of stature as well as weight and for both sexes. Regarding the explanation of this finding, one can not be so certain. I have, in the discussion, assumed the tentative attitude that we were dealing with what could

be taken to *represent* growth curves of the individual. This is the most usual view and was useful to carry our argument along. In making any critical interpretation of the results, however, we must note that, actually, the observations do not refer to an individual, but to different groups of individuals at successive ages. This being so, one is moved to inquire at once how an hypothetical effect on growth correlated with the month of year could show up. If each monthly age group is composed of random individuals of that age, there would be contained in it individuals born evenly during each month of the year. Any differential effect due to particular months would be averaged out, and the difference between one month and the next would be due *only* to one month of ageing, and not at all to the differential effect as between one or another month of the year. The reasoning is quite valid and we must therefore suppose that the successive monthly age groups are also differentiated as regards month of birth. This would be true, if substantially all the measurements were made during a particular part of the year. On that hypothesis children of progressively increasing months of age would have been born at progressively earlier months of the year. If, for instance, the measurements were made largely in September, then children registered as one month old were born in August, those two months old in July, three months in June, etc. Suppose now, there is a differential effect on growth correlated with month of year, January, February, March, etc., having progressively different influences on growth. A child three months old would be different in size from one two months old by an amount representing one month's growth and *also by an amount representing the increment associated with that particular month.* Insofar as this increment changed progressively in magnitude from month to month, the effect on the data as examined here would be to produce just such a wave as we have found.

Unfortunately I have been able to obtain no definitive information regarding the distribution of birth months of the children contained in the Woodbury report. An estimation of which are the months of the year that appear to accelerate and which decelerate growth would presuppose just this knowledge and for that reason, the matter must remain obscure so far as this study is concerned.

An examination of the literature for information relative to seasonal influence on growth yields scattered and not quite consistent evidence, referring mostly to European data. Malling-Hansen (3) distinguishes three seasonal growth periods: 1. August–December, 2. December–

April, 3. April–August. During the first period, he found, occurred the greatest increase in weight and the least in height; the second is a medium period for both height and weight; the third is characterized by rapid growth in height and a minimum in weight. Thus he finds that for the period during which weight is increasing most vigorously, height is least and vice-versa. This contradicts our observations, inasmuch as we found a parallelism in the deviations of height and weight. Malling-Hansen explained his results by assuming meteorological influences on growth emanating from the sun. Camerer (4), who agreed with Malling-Hansen's findings, disputed his explanation. Schmid-Monnard (5) found that height increased most rapidly in the first and second thirds of the year, while weight increased most rapidly in the fall. Combe (6) in a very careful study found that children born in the summer months are taller than those born in the winter season. Mathias (7) found that in the summer, particularly from August to October, increase in weight is greater than in the winter season, and that height increases most rapidly during the first half of the year, particularly in the months from April to June. Bleyer (8) concluded from his studies of the weights of infants that there was an acceleration in the rate of growth from midsummer to late fall, a retardation in winter and a greater retardation in spring and early summer. Porter (9) noted no seasonal differences in height, and found that weight increased in the summer and fall. Holt (10) found in certain data examined by him that the average gain both for weight and height is greater in the six summer months May–November than in the six winter months November–May. Scammon (11) quotes Buffon as finding from the measurements of Montbeillard, that between birth and five years, the gain in height in the winter months was equal to the gain in the summer ones, but that between five and ten years the growth in height was distinctly greater in the summer than in the winter.

The changes that we noted in the means of successive groups from one age to the next may well be due to growth, and partially at least, they must necessarily be so. But logically, they may equally well be due to some selective process that plays systematically on the individuals before they enter the later age group. In the situation we are dealing with, it may for instance be true that during the first year of life, children born during different months experience different hazards of morbidity which affect lastingly their size. Children born in September live their first six months of life exposed to the morbidity

risks of Fall and Winter, whereas those born in March face those characteristic of Spring and Summer. Insofar as differences in childhood illnesses affect later growth these would appear statistically as progressive differentials in children of progressing age, if it were true also that these children were born in successive months. But we have seen already that unless it were true that the successive monthly age groups were also children born in successive months, there is no explaining the appearance of a wave in the data, as an effect on growth. We are therefore left with the broad alternatives of explaining the findings on the basis of a direct effect on growth of conditions that change progressively during the course of the year, such as sunlight, food habits, clothing, etc., or through some early selection correlated with season. In the first case the wave should be evident in an individual followed progressively from age to age, in the second only as a comparison of groups of individuals who were born at different months of the year. I may say that in view of the independent evidence that season *can* affect growth directly and that such an effect *would* result in such a wave as we have found, I think that this is fundamentally the explanation. More important, however, so far as this study is concerned, than any theory that suggests itself is the indubitable fact that a cycle appears in such data as examined here. It must have *some* biological meaning. With ancillary data on the exact time of birth of each child examined, and data on the same child followed progressively, it would be possible to analyze more precisely what that meaning is.

APPENDIX

I

The average of the deviations from the norm for each of the half years concerned is composed of 36 elements, six monthly deviations for each of six years. Let the deviations of the means be $d_1, d_2, d_3 \ldots d_{36}$, the standard deviations of the distributions from which the means were obtained be $\sigma_1, \sigma_2, \sigma_3 \ldots \sigma_{36}$, the numbers in the respective samples $n_1, n_2, n_3 \ldots n_{36}$. Then the standard deviation of the means is given by (1)

$$\sigma_{d_1} = \frac{\sigma_1}{n_1}, \sigma_{d_2} = \frac{\sigma_2}{n_2}, \text{ etc.} \tag{1}$$

in which σ_{d_1} is the s.d. of d_1, σ_{d_2} that for d_2 etc. The values of the individual σ's and the n's are tabled in the original Woodbury report (2) pp. 18–19.

The average of the deviations for each half year is calculated by (2)

$$A.D. = \frac{d_1 + d_2 + d_3 \cdots d_{36}}{36} \tag{2}$$

The standard deviation of a sum is given by (3)

$$\sigma_{sum} = \sqrt{\sigma_1^2 + \sigma_2^2 + \sigma_3^2 \cdots} \tag{3}$$

in which σ_{sum} is the standard deviation of a sum and σ_1, σ_2 etc. the standard deviation of the elements summed.

The standard deviation of a constant multiple of a variable is given by (4)

$$\sigma_{kv} = k\sigma_v \tag{4}$$

in which σ_{kv} is the standard deviation of k times v and σ_v is the standard deviation of v.

Applying (1) (3) and (4) to (2) we have

$$\sigma_{A.D.} = \frac{1}{36}\sqrt{\frac{\sigma_1^2}{n_1} + \frac{\sigma_2^2}{n_2} + \frac{\sigma_3^2}{n_3} \cdots \frac{\sigma_{36}}{n_{36}}^2} \tag{5}$$

in which $\sigma_{A.D.}$ is the standard deviation of the average deviation for six months.

The standard deviation of the difference between the mean deviations of the first and second six months is obtained from the usual formula (6)

$$\sigma_{diff.} = \sqrt{\sigma_1^2 + \sigma_2^2} \tag{6}$$

in which $\sigma_{diff.}$ is the standard deviation of the difference and σ_1 and σ_2 the standard deviation of the elements.

II

The standard deviation of the difference between two proportions such as dealt with in these tables is given by (7)

$$\sigma_{p_1-p_2} = \sqrt{PQ\left(\frac{1}{n_1} + \frac{1}{n_2}\right)} \tag{7}$$

in which $\sigma_{p_1-p_2}$ is the standard deviation of the difference between the two proportions, p_1 and p_2, n_1 is the number in the sample of p_1, n_2 the number in that of p_2,

$$P = \frac{p_1n_1 + p_2n_2}{n_1 + n_2}$$
$$Q = 1 - P.$$

REFERENCES CITED

1. Berkson, Joseph. Growth changes in physical correlation—Height, weight, and chest-circumference—males. Human Biology, I, (1929), 462.
2. Woodbury, R. M. Statures and weights of children under six years of age. Children's Bureau, U. S. Dept. of Labor Publication No. 87, (1921).
3. Malling-Hansen, R. Über Periodizität im Gewicht der Kinder an täglichen Wägungen wahrgenommen. Fragment I. Kopenhagen 1883. Fragment II und III. Perioden im Gewicht der Kinder und in der Sonnenwärme. 1886.
4. Camerer, W. *Jahrbuch für Kinderheilkunde*, XXXVI, (1893), 249.

5. Schmid-Monnard. *Jahrbuch für Kinderheilkunde*, XL, (1895), 84.
6. Combe. *Zeitschrift für Schulgesundheitspflege*, IX, (1896), 569.
7. Mathias. Quoted in Czerny, A. and Keller, A. Des Kindes Ernärung, etc. Leipzig und Wien, 1925, p. 837.
8. Bleyer, A. *Archives of Pediatrics*, XXXIV, (1917), 366.
9. Porter, W. T. *American Journal of Physiology*, LII, (1920), 121.
10. Holt, E. *American Journal of Diseases of Children*, XVI, (1918), 359.
11. Scammon, R. E. *American Journal of Physical Anthropology*, X, (1927), 333.

A METHOD OF COMPUTING THE CROSS-SECTION OF THE CHEST FOR CHILDREN AT SEVEN, EIGHT, AND NINE YEARS[1]

BY GRACE CLARK[2]

In studying a series of physical measurements made on a group of children over a period of years, one of the foremost problems presenting itself is that of the value of such a series as an indicator of growth. A comparison of the rates of growth of certain parts of the body is of value, but the relation existing between the development of these parts and the development of the body in general invites attention particularly.

This study is a small part of an investigation begun by Dr. Bird T. Baldwin, late director of the Iowa Child Welfare Research Station (1), whose purpose was to study physical growth from birth to maturity by the consecutive method. The children measured were those who had attended the observational school of the State University of Iowa. From this group eleven children who had been measured at regular intervals from seven to nine years of age inclusive were selected to investigate the relation existing between the actual area of the segment of the chest at the level of the nipple line and the same area computed by means of an empirical formula based on chest width, depth, and circumference measured directly on the living subject.

DEVELOPMENT AND VERIFICATION OF AN EMPIRICAL FORMULA FOR ARRIVING AT A COMPOSITE CHEST MEASURE

Since the major purpose of this study was to determine the relationship existing between this segment of the chest and general body build, one of the important problems was the derivation of a composite measure expressing the area of a section of the chest from the three available chest measurements, width, depth, and circumference.

[1] This study was directed at the Iowa Child Welfare Research Station by Sarah Idell Pyle and Helen Garside Kelly.

[2] Clark, Grace: The Relation of the Growth of a Segment of the Chest to General Body Growth. Iowa Child Welfare Research Station. Unpublished Master's Thesis, 1929. Pp. 87.

Various considerations of the geometric figure to which this segment of the chest conformed most closely were entertained.

Development of the Formula

The first assumption was that, from observation, the chest is roughly elliptical in shape. The validity of this assumption was tested by computing the circumference of an ellipse, using the formula $2\pi\sqrt{\frac{a^2+b^2}{2}}$, where a represents $\frac{\text{chest width}}{2}$, and b, $\frac{\text{chest depth}}{2}$. The average difference between the true circumference as measured and as computed was 4.9 cm. for boys whose average chest circumference was 53.8 cm., and 4.9 cm. for girls whose average chest circumference was 58.3 cm.; the true circumference of the chest being larger.

The next assumption was that the shape of the chest conforms somewhat to a rectangle, the sides of which are represented by the chest width and depth. When the perimeter of such a rectangle was compared with the true circumference of the chest, the average difference was 8.5 cm. for boys and 8.7 cm. for girls, the perimeter of the rectangle being larger than the true circumference of the chest.

From these results it would seem that the chest conforms to a figure combining the characteristics of a rectangle and an ellipse. Such a figure would be the parallel octagon, which from the point of view of this study, represents a rectangle from which the corners have been cut. Based on these assumptions, an empirical formula was developed, the derivation of which is given below:

Let AB = chest width
Let BC = chest depth
Let chest cir. = chest circumference

$$\text{Chest cir.} = 2(AB + BC) - 8a + 4\sqrt{2a^2}$$
$$\text{Chest cir.} - 2(AB + BC) = -8a + 4\sqrt{2a^2}$$
$$2(AB + BC) - \text{chest cir.} = 8a - 4\sqrt{2a^2}$$
$$2(AB + BC) - \text{chest cir.} = 8a - 4a\sqrt{2}$$
$$2(AB + BC) - \text{chest cir.} = 4a\,(2 - \sqrt{2})$$
$$4a = \frac{2(AB + BC) - \text{chest cir.}}{2 - \sqrt{2}}$$
$$a = \frac{2(AB + BC) - \text{chest cir.}}{4(2 - \sqrt{2})} = \frac{2(AB + BC) - \text{chest cir.}}{8 - 4\sqrt{2}}$$

Area of one of the four equal triangles $= \frac{a^2}{2}$

$$\text{Area of chest} = \text{area } ABCD - 2a^2$$

$$\text{Area of chest} = \text{area } ABCD - 2\left(\frac{2(AB + BC) - \text{chest cir.}}{8 - 4\sqrt{2}}\right)^2$$

$$\text{Area of chest} = AB \times BC - \{.603553[2(AB + BC) - \text{chest cir.}]\}^2$$

Verification of the Formula

The validity of the empirical formula was determined in the following way. Eleven children, five boys and six girls, representing different types of stature, were chosen from the seven, eight, and nine year groups. Types of stature were selected according to the Baldwin-Wood's tables (2), (1) slender short, denoting a condition of under-weight and under-height; (2) average short, average weight, and under-height, (3) slender medium, under-weight and average height, (4) average medium, average weight, and average height, (5) average tall, average weight and above-height and (6) heavy tall, over-weight and above-height. The various stature types were chosen in order to verify the use of the empirical formula for different types of build.

The shape of each child's chest was obtained by means of two sections of lead tape, about one-sixteenth inch in thickness and one-fourth inch in width. To insure a quicker and more accurate measurement, two persons worked on the experiment. The child was given no directions but was merely asked to stand still. The ends of the lead tapes were placed on the midline of the sternum, at the level of the nipple line. The tapes were then passed backward around the chest and brought together over the tip of the spinous process of the thoracic vertebra corresponding to this level. As soon as the tapes were properly placed the child was asked to raise his arms slightly and the two pieces of the tape were removed and fitted together, care being taken to preserve the exact shape until it could be transferred to a paper and traced off. Three such measurements were made for each child. Weight, height, breathing capacity, width, depth, and circumference of chest were also measured at the time.

A Coradi disc planimeter was used in measuring the areas of the traced chest shapes. Because it was difficult to measure such a large area with the planimeter, circles were inscribed within the traced shapes and the areas of the circles were computed from their radii; the irregular spaces between the circles were measured by the planimeter. Three readings were taken for each of the irregular spaces within the traced shapes and the average of the three was taken for

the final dimension of each space. The sum of the areas of the circles, plus the areas of the irregular spaces, gave the total chest area.

The next step was to determine whether the area computed by the formula corresponded to the actual area of the chest as measured by the planimeter. The ideal method would have been to measure the chest circumference, width, and depth with the obtaining of the chest shape at the moment when the chest shape was being measured with lead tapes, and to compare the area derived from the measurements with the area of the traced chest shape. This was impossible because as slight a movement as that of lifting the arms to have either the calipers or the measuring tape placed on the chest wall under the arms would alter the shape of the chest. Hence the chest shape was measured independently as traced off on paper. The greatest width of the shape was used as chest width; chest depth was taken from front to back where the tapes met; and chest circumference by passing a metal wheel (devised by Wellman (4)) twenty centimeters in circumference around the traced chest shape. By substituting these three measurements in the formula, a was computed (a being one of the two equal sides of the isosceles triangles, four of which were removed from the rectangle to form a parallel octagon). Since the area of two isosceles triangles is equivalent to the area of a square, the area of a square whose sides were a was computed. This area multiplied by two represented an area equivalent to the sum of the area of the four triangles. The chest area was then computed according to the formula for the area of a rectangle. The total area of the triangles subtracted from the area of the rectangle gave the area of the parallel octagon.

Table 1 shows the per cent of difference between the area as measured by the planimeter and that computed by the formula for each of the three series of measurements for each child. The means, standard deviations, probable errors, probable error of the difference and ratios of the mean areas computed by the two methods are given below:

	Children	*Planimeter Area*		*Computed Parallel Octagon Area*		*Probable Error of Difference*	*Ratio*
		Mean	$P.E._M$	Mean	$P.E._M$		
1st measurement...	11	244.41	5.86	242.40	5.78	8.23	.26
2nd measurement..	11	248.35	5.27	246.36	5.31	7.48	.27
3d measurement...	11	248.32	5.75	247.18	5.58	8.01	.14

TABLE 1

Difference between Chest Area Computed by the Empirical Formula for the Area of a Parallel Octagon and Actual Chest Area as Measured by a Disc Planimeter

CHILD'S NUMBER	SEX	AGE	STATURE TYPES*	1ST MEASUREMENT				2ND MEASUREMENT				3D MEASUREMENT				MEAN PER CENT OF DIFFERENCE
				Chest Area by Planimeter	Chest Area by Formula	Difference	Per Cent of Difference	Chest Area by Planimeter	Chest Area by Formula	Difference	Per Cent of Difference	Chest Area by Planimeter	Chest Area by Formula	Difference	Per Cent of Difference	
1	M	8	Slender short	232.22	237.66	5.44	2.3	237.87	242.20	4.33	1.8	244.10	244.17	.07	.03	1.4
2	F	8	Slender short	227.44	223.85	3.59	1.6	225.44	221.02	4.42	2.0	229.84	224.52	5.32	2.3	2.0
3	F	7	Average short	226.95	220.71	6.24	2.7	228.69	227.08	1.61	.7	229.02	230.00	.98	.4	1.0
4	M	9	Slender medium	225.47	223.65	1.82	.8	226.07	220.96	5.11	2.3	229.68	226.73	2.95	1.3	1.4
5	F	9	Slender medium	225.46	223.91	1.55	.7	246.47	247.70	1.23	.5	226.08	229.22	3.14	1.4	.4
6	M	9	Average medium	292.24	291.95	.29	.1	289.46	287.83	1.63	.6	291.68	290.70	.98	.3	.3
7	F	7	Average medium	240.76	235.89	4.87	2.0	240.63	236.30	4.33	1.8	224.08	241.11	2.97	1.2	1.7
8	M	9	Heavy medium	305.76	302.29	3.74	1.2	299.62	298.48	1.14	.4	292.37	301.75	9.38	3.2	.5
9	M	7	Average tall	225.54	222.59	2.95	1.3	232.79	228.91	3.88	1.7	239.31	231.47	7.84	3.3	2.1
10	F	7	Average tall	218.62	220.64	2.02	.9	227.68	227.63	.05	.02	223.29	221.80	1.49	.7	.07
11	F	7	Heavy tall	269.14	262.95	6.19	2.3	277.14	271.90	5.24	1.9	282.02	277.54	4.48	1.6	1.9

* The classification of the stature of the children was made on the basis of the Baldwin-Wood standards.

TABLE 2

Difference between Chest Area Computed by the Mathematical Formula for the Area of an Ellipse and Actual Chest Area as Measured by a Disc Planimeter

CHILD'S NUMBER	SEX	AGE	STATURE TYPES*	1ST MEASUREMENT				2ND MEASUREMENT				3D MEASUREMENT				MEAN PER CENT OF DIFFERENCE
				Chest Area by Planimeter	Chest Area by Formula	Difference	Per Cent of Difference	Chest Area by Planimeter	Chest Area by Formula	Difference	Per Cent of Difference	Chest Area by Planimeter	Chest Area by Formula	Difference	Per Cent of Difference	
1	M	8	Slender short	232.22	220.35	11.87	5.1	237.87	229.34	8.53	3.5	244.10	231.50	12.60	5.1	4.6
2	F	8	Slender short	227.44	219.75	7.69	3.3	225.44	225.72	.28	.1	229.84	225.06	4.78	2.8	1.8
3	F	7	Average short	226.95	224.75	2.20	1.0	228.69	227.42	1.27	.5	229.02	207.85	21.17	9.0	3.5
4	M	9	Slender medium	225.47	203.26	22.21	9.0	226.07	204.24	21.83	10.0	229.68	206.06	23.62	10.0	9.9
5	F	9	Slender medium	225.46	192.14	33.32	10.0	246.47	224.81	21.66	8.8	226.08	208.29	17.79	7.8	10.0
6	M	9	Average medium	292.24	283.78	8.46	2.9	289.46	276.40	13.06	4.5	291.68	254.38	37.30	13.0	6.7
7	F	7	Average medium	240.76	228.24	12.52	5.2	240.63	231.03	9.60	4.0	244.08	231.85	12.23	5.0	4.5
8	M	9	Heavy medium	305.76	270.52	35.24	12.0	299.62	268.67	30.95	10.0	292.37	273.48	18.89	6.0	9.4
9	M	7	Average tall	225.54	192.67	32.87	14.0	232.79	201.19	31.60	13.0	239.31	213.88	25.43	10.0	12.0
10	F	7	Average tall	218.62	205.21	13.41	6.0	227.68	206.75	20.93	9.0	232.29	205.18	27.11	12.0	9.0
11	F	7	Heavy tall	269.14	236.48	32.66	12.0	277.14	246.93	30.21	11.0	228.02	253.28	25.26	11.0	10.8

* The classification of the stature of the children was made on the basis of the Baldwin-Wood standards.

From Table 1, demonstrating the small per cent of difference between the areas measured by the planimeter and those computed by the formula for each of the three series of measurements for each child, it would seem that the empirical formula for finding the area of the chest is a valid method of arriving at a composite chest measure. It should be noted, however, that the small number of cases probably affects the reliability.

As a further means of verifying the use of the empirical formula for computing chest area, a comparison may be made between Tables 1 and 2. Table 2 shows the per cent of difference between the areas as measured by the planimeter and the areas computed by the formula for the area of an ellipse, which is π a b. The probable errors, probable errors of the difference and the ratios of the mean areas are given.

	Children	*Planimeter Area*		*Computed Ellipse Area*		*Probable Error of Difference*	*Ratio*
		Mean	P.E.$_M$	Mean	P.E.$_M$		
1st measurement...	11	244.51	5.86	225.19	5.72	8.18	2.4
2nd measurement..	11	248.35	5.27	231.13	4.75	7.10	2.4
3d measurement...	11	244.22	4.73	228.25	4.52	6.54	2.4

Inspection of these results shows a much higher per cent of difference than in the case of the use of the empirical formula. The ratios, however, indicate only a tendency toward significant differences in the mean areas. As noted above, the small number of cases probably affects the reliability.

If then we judge the applicability of those methods of computing chest area by the per cent of differences found in the measurements of individuals, the use of the empirical formula,
area $ABCD = AB \times BC - \{.603553[2(AB + BC) -$ chest cir.$]\}^2$,
seems the most valid method of arriving at a composite chest measure.

SUMMARY

Theoretically, the most accurate method of finding the area of the chest would be by measuring the traced chest shape with the planimeter. It is impractical because too much time is used in getting the chest shapes by means of lead tapes, and then measuring the areas with a planimeter. When the series of three chest shapes for each child was analyzed the errors made in getting the chest shapes with the lead tape were larger than the errors made in measuring with the

planimeter. It is obvious that the shape of the chest varies too much to warrant measuring with a delicate instrument such as the planimeter.

Practically the empirical formula fills the need for a method by which we can compute an area of the chest approximating closely the actual shape of the chest in the living child at seven, eight, and nine years of age for the following reasons:

(1) In the case of the eleven children for whom three chest shapes each were measured, the small per cent of difference between the areas computed with those measured by the planimeter shows that the shape of the segment of the chest in children at seven, eight, and nine years conforms closely to the shape of a parallel octagon. Hence, the empirical formula for it can be used to compute the area of this segment.

(2) The empirical formula is a time saver.

REFERENCES

1. Baldwin, Bird T.: The physical growth of children from birth to maturity. *Univ. Iowa Stud., Stud. in Child Welfare,* 1921, 1, No. 1, Pp. 411. (p. 117–137.)
2. Baldwin, Bird T., and Wood, Thomas D.: Weight-height-age tables in English units of American-born boys [girls] [clothed] of school age. Iowa Child Welfare Research Station, 1927. Revision and expansion by Helen V. Garside.
3. Hrdlicka, Ales: Anthropometry. Philadelphia, Pa.: Wistar Institute of Anatomy and Biology, 1920. Pp. 163. (p. 76–77.)
4. Wellman, Beth: The development of motor co-ordination in young children: An experimental study in the control of hand and arm movements. *Univ. Iowa Stud., Stud. in Child Welfare,* 1926, 3, No. 4, Pp. 93.

DISTURBANCE OF VISION AND CONSCIOUSNESS IN PETIT-MAL ATTACKS

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The fact that the epileptic usually is unable to give us an account of what happens during a fit stands in remarkable contrast to the highly organized activity we observe in some of the attacks. It is a generally accepted theory that the loss of consciousness forms the nucleus of the epileptic attack. There are only a few attempts at the analysis of these unconscious states. With the method of narrowing and focussing the attention and lines of remembrance in hypnosis, one has a means of penetrating to a certain extent further into what otherwise may not formulate itself in communicable experience. This type of approach had been tried by Binder in Heidelberg, who investigated parts of an epileptic fugue with hypnosis and found a marked disturbance in vision. The characteristic features were concentric restriction and disturbed fusion which he describes with Mayer-Gross as "dilapidating consciousness". Frequently he found micropsia and double vision. During the fugue the patient had marked fluctuation of consciousness (according to the hypnotic reexperience) but he had nevertheless a complete amnesia for the whole episode.

Our investigations were limited to petit-mal attacks which seemed to be of only a few seconds' duration with apparently no visual or other experience during this loss of consciousness. Under hypnosis the patient reproduced memories of larynx and eye feelings and of his general behavior as well as characteristic disturbances of the visual field—an unexpected definition of experiences behind a seemingly empty unconscious period. Petit-mal attacks of several patients were investigated and gave similar results, but one patient whom I shall use as the *paradigma* gave especially striking results.

This patient was a 30 year old college professor who has petit-mal attacks since the age of 24. They became more frequent the year previous to admission. His intelligence was intact at admission

to the Henry Phipps Clinic and except for a slight change in personality, the petit-mal attacks were the only symptoms of epilepsy. The attacks occurred two to three times a week and eighteen petit-mal attacks were examined.

The patient was closely observed, the nurses turning in accurate reports about each attack. The patient told the physician his own observations, but they were not discussed with him. He was then made to rehearse the attacks scene by scene under hypnosis. Two levels of hypnosis were used for each scene, firstly an average deep hypnosis, then a deep hypnosis. Each scene was investigated completely before the succeeding one was brought under investigation. Special attention was paid to the patient's behavior at that moment, his bodily sensations and his visual field. He was then put in the deeper hypnosis and the same scene was examined with regard to the same factors, and in addition to them, with regard to the thought content. In this manner, the whole attack was examined step by step. We soon found out that we did not deal with an abrupt, brief attack. Sensations may precede or last for several minutes after the attack, accompanied by a varying degree of disturbance of consciousness. It took us therefore frequently two hours for the examination of a brief petit-mal attack. Verbatim reports were taken by another member of the staff.

A CASE REPORT

The following report will give an understanding of our findings. The nurse observed during breakfast that the patient, while taking a drink of water, spilled a little on his chin. His hand jerked a little and his fingers became rigid. He nodded his head a few times stiffly and his jaws moved as if chewing. The eyes looked glassy and his gaze was fixed straight ahead. He then dropped his right hand and the whole attitude relaxed. After about a minute he reached behind him to take a cigarette which was in a cigarette holder on the windowsill. He took matches, lit the cigarette and held the holder in his mouth, very rigidly. His eyes remained half open and his movements were very slow. He remained for several seconds with the match held in his fingers, the arm straight and rigid, the face flushed, the cheek muscles looking taut. Finally he opened his eyes, smiled at the nurse, then looked over the table as if he were taking in all details and felt puzzled by it all, righted his chair and resumed eating his breakfast. This is as far as the report of the nurse, the objective

observer, goes. The patient himself remembers that he had an attack of short duration. He realizes that he must have taken a cigarette during the attack and lit it but he cannot remember it. He recalls that he felt puzzled the moment before he became himself again.

We started with the hypnotic investigation at the moment when he reached for his egg, which happened a few seconds before the attack started. The first scenes are omitted as nothing unusual was noticed and the protocol starts with the description of the first sensations:

There is a slight sensation in the throat. I am looking at this particular moment at the bowl before me; there was some white cereal into which I had mixed an egg.

Next Scene:—I still see that bowl, with myself picking at it in a way with my fork. I have a sensation of lassitude, put my fork down at the right place. I see things now not as a whole but only that towards which my sensations are directed. I have slight sensations in my fingers.

Next Scene:—I seem to see the glass of water. The sensations are growing stronger. I can see myself holding the glass. I am sitting erect at the table, but I have the feeling that I am tipping back in my chair.

Next Scene:—I don't seem to see much but there is a tightening of the throat muscles, especially in the vocal region. My visual field tends to be luminous like a moving picture screen. A picture of a man comes into my mind. It is a man who is a colleague of mine. I seem to see him with a cigarette. It takes me back to my office, though it is in a different condition than when that man was there.

Next Scene:—There is a degree of numbness in the nasal region, a tendency to dilate and contract the nostrils. The visual field is dark or grey with a tendency to luminousness. Again there is that concentric affair, it now assumes the form of a grille in our kitchen.

Next Scene:—There is a tendency to nasal movements. It is a twitching. The arms are fairly normal. There are slight sensations in the arms. There is a ball of something black with light in the center passing out in the form of streaks. It is a bluish light. There are no other colors, just luminously.

Same Scene—Deeper:—It seems black. I have a feeling that I tip back. I have a feeling that I reach round behind me to the window-sill for the cigarette that I had left there. There is a general idea of being puzzled. I feel that my head has rocked.

Next Scene:—I still have a slight nasal twitching, a slight throat sensation, only slight hands sensation. I cannot see things clearly. I can see the cigarette holder that I was given the other day. I do not know whether it was really there or whether it was my imagination. I seem to see the end of it containing the cigarette. I take the other end in my mouth. I am sitting in a fairly normal posture. I can see a large trumpet shaped flower with the open end towards me. It is of a dark color. I want to see the entrance of Miss A's (his fiancée) room where I used to go some times. It is the entrance as one would view it from the inside going out. The thing that seems to be hovering around is my driving the car at a certain time in a certain direction. I was in a rather peculiar mood. I had

just been speaking to Miss A and had just been disappointed in a certain way, about my proposal. It was in her room.

Next Scene:—I still have throat sensations and slight numbness. I want to see the table cloth in front of me, and a box of matches on it. I am sitting normally, can see things quite normally. I can see the table.

Next Scene:—There seems to be slight throat sensations. I seem to see the nurse in front of me. Just at this moment she seems blurred. She is just what I happen to be looking at. I can localize her in the room.

Next Scene:—I have a slight sensation on one spot on my throat. It is on the left side, slightly below the voice box. I see the nurse pretty clearly sitting there looking at me in an interested attitude. There seems to be a persistence of the blurred stage though I have a fairly good outline. It seems to me that I am bringing my chair down to the floor again after having tipped it back. I have the cigarette in my hands. It seems to be dead, but that may be a continuance of what I saw earlier in the morning.

Next Scene:—I have slight sensations. While I can keep the image of the nurse fairly well a lot of things tend to interfere. I can see her clearly. I see myself straightening up in the chair.

Next Scene:—I have very slight sensations in that spot in the throat. It seems to me that I turned around to the table. I do not know whether I eat or not.

Next Scene:—There seems to be a slight persistance in the throat. I can take in the visual picture better. I am at the table—there is no one there.

Next Scene:—I am leaving the dining-room, considering myself normal, though with sensations in my throat. Things are a little shadowy but I can localize fairly well.

Next Scene:—I am seated somewhere. I can see things clearly. I still have feelings in the throat; I did not feel it then.

Next Scene:—There is only that throat sensation. I approach someone and ask Mr. S. (another patient) whether I have not fallen asleep. I see him clearly.

Next Scene:—I seem to feel normal. He said that he left when I lifted a glass of water. I went on into the day-room.

The examinations of other attacks gave similar findings which I may summarize in the following facts:—a petit-mal attack has its characteristic changes in consciousness and vision and bodily sensations. In all attacks there were sensations of tightness, tenseness and numbness. They were noted more frequently in the throat and face, less frequently in the hands and neck and not elsewhere. They appeared at a period before the attack which varied from about two hours to a few minutes, and lasted for a varying period up till ten minutes after the attack. They showed a variation in intensity throughout their appearance which was not correlated with any variation in consciousness. In deep hypnosis these sensations were frequently more intense. The tight feeling in the throat was often so marked that his speech became thick, low and nearly unintelligible. Twitching of muscles was observed. The patient never experiences

these sensations in his conscious state and never observed them with his petit-mal attacks. They were only found under the hypnotic investigation. So far from the unconsciousness of the attack being an absolute and complete thing, we found that it was very fluctuating. At times there was almost a clear consciousness, at others a vague appreciation of surrounding objects, passing at some points into a complete loss of perception of outside objects.

During less marked disturbance of consciousness the patient was able to describe his behavior in average deep hypnosis. With deeper hypnosis he was able to give a more or less vague picture of himself even during the so-called unconscious period, but there was usually a brief episode which the patient was unable to reproduce. This must correspond to a real unconscious state, best compared to coma.

DISTURBANCES OF VISION

Vision has to be studied in regard to the formal aspect as well as to content. There is a definite concentric constriction of the field of vision on entering the attack and a similar concentric enlargement on coming out. The patient never described a change in the size of the objects but saw them on one occasion upside down. On some occasions he noticed that objects seemed to move slowly and this was often associated with a slowing down of his own activities. As the attacks progressed, his attention became gradually more fixed on the central objects and the sense of the situation and location was lost.

The following samples from various attacks illustrate well this disturbance of fusion:—

I am trying to see myself. I feel this is a deeper state than the one before. I feel that I am being lifted up. There may be a tendency to disordered pictures, parts of the furniture, incongruous parts of the room, which is not reality.

I can see only a face without any contacts of surroundings. It seems he is in the attitude of sleeping. It is the face of Dr. Meyer.

I am sitting. I am not conscious of my hands. I can see parts of the chair . . . I can see things immediately at my sides, such as the white of the pillow and the bed table . . . in front of me it seems everything is black. I have the picture of slow motion. Something is revolving. I can't tell what it is. It is dark and does not persist particularly.

I am holding the program. It still seems to me that I am looking at that cover part of the program. I can see the type of character used, though I can't see the words. It seems to me that I can see extra letters.

I still see the needle and the double line. I can see the needle moving to the right. I am sitting at the table rather tense. I am not concerned with the rest of the room. I seem to see books on the table. I seem to see the window. The next

thing that comes into my mind would be the lamp. There is always a tendency to other things, leaves, etc . . . I have the idea of a window through which something is hanging, tapestry, a wall is surrounding that window.

Situations and the whole changing sight are not experienced as a fused pictorial memory as in the normal person, but as a succession of pictures. Often, only several single objects are seen but not in relation to each other and to the picture as a whole.

The above mentioned phenomena (concentric constriction of the visual field, restriction of attention to the central objects, loss of the sense of the situation and location, disturbance of fusion) were present only so long as the patient's consciousness was still on a level where he could perceive optic stimuli. During the depth of the attack, external stimuli were not perceived any longer. The visual field was filled with pictures the origin and nature of which is more difficult to explain. In medium hypnosis such a scene showed mainly a play of light and darkness, occasionally taking concentric forms. They correspond to the pictures of entoptic origin as recently described by Hoche. In deep hypnosis concentric pictures predominated. They were often colored, e.g., as flowers and trumpets. Not infrequently whole landscapes were seen. Many of the pictures were related to his past life, especially experiences with his fiancée. These findings are very different from those which we got from hypnotic examinations of normal periods of this patient's life and from those in normal people. Normal scenes are reexperienced according to reality and the visual field is filled with the objects of the surroundings. We find occasionally entoptic pictures like some play of light and darkness, often of concentric shape when people are relaxed and given to self observation as in psychocatharsis and hypnagogic hallucinations.

During our investigation we were always very guarded against making any suggestions to the patient, but I realize that in the restoration of amnesic gaps associated material may interpose. The individual reaction can be clearly seen in the patient's reproduction and deception may arise easily. I therefore exclude a discussion of the interesting content and try to evaluate the more non-personal factors. In all these experiments we never got hold of an initiating factor but were at least able to establish a closer observation of the by no means wholly inaccessible attacks of relative unconsciousness.

Another patient with petit-mal attacks, an 18 years old college boy, showed an interesting visual disturbance under hypnosis. With the beginning of the attack, objects on which he had focused became

gradually "smaller and far away", till everything seemed black. With restoring consciousness these objects became "as large as they would be as if I were close". Sometimes such an object seemed very large and filled the whole visual field.

Recently a patient who did not become unconscious and amnesic in peculiar attacks with interesting visual changes gave descriptions not unlike what in the other cases could be brought out only in hypnosis.

This patient, a lawyer 41 years old, had suffered from convulsions since the age of 29. Since April, 1927, he has had attacks which consist in two weeks of visual disturbance, followed by a series of convulsions lasting three days. During the visual disturbance he does not lose consciousness. We observed him during these attacks and careful notes were taken of his descriptions. The visual disturbances lasted from a few seconds to minutes. At the onset and coming out of some of these attacks he saw only objects which were in the center of his visual attention. This often led to distortion. Then the visual field became filled with colors which during the depth of the attack changed to grey. Occasionally we observed petit-mal attacks with loss of consciousness. Hypnotic investigations of these attacks revealed findings similar to those I have just described. They were accompanied by palpitation and a dazed feeling, ending suddenly with the clearing up of the attack. Also here we found fluctuations on different levels of consciousness.

During his conscious attacks he often saw objects very small or very large or upside down. Hallucinations of tall trees, boxes, wires and the walls moving towards him occurred frequently. An eye examination revealed a homonymous hemianopsia during the attack, and normal findings otherwise.

In this case we obtain disturbance of vision similar to those in my experiments. I would like to point out the disturbance in vision through restriction of attention and the colorful pictures with the more marked loss of consciousness. There were also sensations in the hypnotic re-experience of which he was not aware before.

With these hypnotic investigations we are trying to attack the problem of unconsciousness in the epilepsy reactions. The results allow us to single out a few factors which are more intelligible than any concept of consciousness as such. Instead of this we found the more familiar problems of attention and vision. Hypnosis allows us to analyze "even mild petit-mal attacks in which normal consciousness is commonly recovered so quickly that no sufficiently long inter-

mediate one persists during which the patient can be induced to talk." (Clark). Through the limitation of attention we get a disturbance of fusion. The situation picture does not exist any longer and the patient can no longer "include the usual wide range of facts within the optic reach". He often does not fuse the continually changing picture "into one total picture". During the more marked disturbances there is no longer "a fusion of successive impressions." The "active looking is replaced by passive seeing". Primary symbolizations fill the visual field and "memories of past experiences and imaginative productions" play an important role, showing more clearly than in the conscious state that "vision is an actual reaction of the individual as a whole." (Adolf Meyer.)

NOTES

REMARKS ON THE PAPERS BY PROFESSOR F. WOOD JONES AND PROFESSOR W. K. GREGORY CONCERNING THE PHYLOGENY OF PRIMATES

The investigator has three methods at his disposal with which to clarify the phylogenetic relations of animal forms. These are comparative anatomy, palaeontology and embryology. For the investigation of the genealogy of Primates, the first two methods have been almost exclusively used until now. The discussions of Prof. Wood Jones in the May, 1929, and of Prof. Gregory in the February, 1930, number of HUMAN BIOLOGY are based only upon comparative anatomy and palaeontology. Only after Hubrecht and Selenka had secured the necessary material was it possible to investigate the comparative embryology of Primates.

In the past few years, the author thoroughly occupied himself with the comparative embryology of Primates, especially the embryology of the skull of various Primate forms (*Tarsius, Nycticebus, Lemur, Hapale, Chrysotirix, Mycetes, Macacus, Semnopithecus, Homo*). These were examined mostly on hand plastic reconstructions (1). The embryology of *Tupaja* was also investigated (2), as Gregory (1910, 1913) has brought this Insectivore into closer phylogenetic relation to the lower Primates.

If one compares the embryonic cartilaginous skull of a human embryo with that of an old world monkey, the morphological similarity of the entire construction is extraordinary. Upon more exact consideration, one comes across a few characteristics with regard to which differences exist (course of the skull axis, etc.). If the primordial cranium of the Cercopithecidae is compared with that of the new world monkey, the similarity is still greater. It is noteworthy that *Hapale* is closely related to the examined Cebidae. This almost complete accord of the primordial cranium of old and new world monkeys must be considered as evidence of their close genealogical relationship. It is apparent that old and new world monkeys have a common origin, since that is the opinion of Haekel, Boas, Winge,

Dubois and Schlosser, contrary to that of Loomis, Gregory, von Ihering, Franz and Schwalbe. The Hapalidae do not receive a special position opposite the Cebidae, as Wortmann, Arldt, Gidley and others suppose. They belong genealogically with the new world monkeys according to the conception of Bluntschli, Gregory and Franz.

Upon investigation of the skull development of lower Primates it becomes apparent that *Tarsius* and Lemuroidea (*Nycticebus, Lemur*) retain characteristics basically different from one another. This embryologic finding is not surprising, since it has been known for a long time that *Tarsius* anatomically is not closely related to the rest of the lower Primates (lateral enclosure of the orbits, Tympanicum; failure of a proc. lemuricus mandibulae, deciduate Placenta, Caecum, failure of the Prosimian loop of the Colon). We are partly concerned here with characteristics which *Tarsius* and the higher Primates commonly possess. However only outer relationships are shown (for example with regard to the placentation (von Möllendorf)) whose phylogenetic worth cannot be judged. With *Tarsius* the evolution of the skull shows not only no approximation to the higher Primates, but deviates the most among the Primates from the general type. *Tarsius* and the known fossil Tarsioidea are entirely one sided specialized forms. Therefore I can not imagine an origin of the higher Primates from similar Tarsius forms, contrary to the opinion of Wood Jones, Cope, Hubrecht, Schlosser, Franz, Teilhard de Chardin, Arldt, E. Smith and others. Since the primordial cranium of the investigated Lemuroidea (*Nycticebus, Lemur*) approaches the higher Primates as a whole, just as in their most important individual characteristics, I consider that the origin of the latter is from forms which are similar to the present Lemuroidea.

Another genealogical question, which is affected by embryological results, is that of the origin of the entire group of Primates. Mammals similar to Insectivora are mostly thought to be the origin (Franz, Boas, Weber, Haeckel). Gregory states more definitely here (1910, 1913) that the Primates as a whole originate from a form similar to Tupaja. Gregory continues on that numerous characteristics of Tupaja are distinguished from the Lipotyphla; the neurocranium is more spacious, the nasal region of the skull is reduced as among Primates, the orbit is comparatively large, it being bordered by a bony ring as with the Lemuroidea. A Foramen infraorbitale is present, and the form of the auditory ossicles, the tympanic region

and the direction of the arteria Carotis interna, show approximation to the lower Primates. A lower tongue is present as with the latter. Gregory was therefore convinced that primitive Insectivora Menotyphla are the origin of Primates. This opinion was shared by Schwalbe, Ossenkopp, Le Gros Clark, Carlsson, E. Smith. Before Gregory, Kaudern had already come to the same conclusion.

In reality Gregory's assumption bases itself upon the similarity of the skeletal system, especially the adult skull. What is the relationship of the embryological history of the skull to it? It becomes apparent upon investigation that the primordial cranium of *Tupaja* deviates completely from the Primate type and is closely related to the other Insectivora. According to the entire contour form, to the general construction, to the direction of the skull axis, to the passage of the arteria carotis interna, to the Parietal plates, to the form of connection between the nose capsule and the skull base, according to this, the skull evolution is entirely different from that of the Primates and this form shows fundamentally the same ontogeny as the other investigated Insectivora. A genetic relation between Tupajidae and Primates is to be rejected. The origin of Primate genealogy remains uncertain.

So we can see, that the embryological method is in a position to provide real contributions to the phylogeny of Primates. It was the aim of the preceding text to show the worth of embryological investigation for the genealogical history and the embryological reports, from which up to this time investigated results deviated, should be placed in discussion in their phylogenetic meaning.

1. Studien über das Primordialkranium und die Stammesgeschichte der Primaten. Morphologisches Jahrbuch 59. 1928.
2. Das Primordialkranium von Tupaja und der Ursprung der Primaten. Zeitschr. Anat. u. Entw. 86, 1928.

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PROPOSAL FOR THE COMPULSORY ANTHROPOLOGICAL STUDY OF EMIGRANTS

I should like to propose that the permission for emigration, which already depends upon medical examination, should also be preceded

by an anthropological examination. In other words, my proposal is to annex to the office of medical examination an anthropologic one.

In many ways such an examination would be of great value to science, as a simple means of securing anthropological data on a large number of persons. For Germany the following numbers would be examined:

1921	22,700	1922	36,500
1923	115,000	1924	58,000
1925	63,000	1926	65,000
1927			61,000

The number of emigrants from other countries will not be much smaller. Dividing the examined persons according to country of birth and lineage, the anthropology of each country will be investigated more thoroughly than would be possible in any other way, for there will not be many, who will voluntarily submit to examination.

In the second place the United States would have copious material, by combining the measurements with the medical data, for the study of questions of race and biological heredity. If the examination were repeated after a certain time, the question of the influence of the atmosphere, the milieu, etc., on the frame could be studied thoroughly. This is a question about which for want of material little is now known.

The financial question would not be of great importance. Eventually the persons examined could be required to pay the costs of their examination. The elaboration of the collected material could be accomplished on the spot, or in the anthropological institutes of the different Universities. In the latter case, the material would serve not only for studies, but at the same time for instruction.*

MAX KÄSSBACHER
SCIENTIFIC COLLABORATOR
OF THE ANTHROPOLOGICAL SECTION
OF THE ANATOMICAL INSTITUTE
OF HEIDELBERG UNIVERSITY

* A similar proposal has been made by Dr. H. H. Laughlin.—EDITORS.

QUOTATIONS

AN EIGHTEENTH CENTURY FRENCH EVOLUTIONIST

One of the major curiosities of biological literature is the *Vue Philosophique* of J. B. R. Robinet. It is now a rare book. Its significance in the pre-Darwinian discussions of the evolution concept has been appraised, on the whole fairly, by Osborn,[1] but it seems worth while to give an opportunity to the present day biologist to see at first hand something of what Robinet actually said about the struggles of nature to make man.

Jean-Baptiste-René Robinet was born in Rennes on June 23, 1735, and some eighty-five years later died there, on March 24, 1820. In the long interval between these two events he worked his way through a somewhat difficult and troubled life. In 1735 Rennes was near a low ebb in its fortunes. Though it had been, in effect, the capital of Brittany since the 10th century, it had during the preceding fifty years or so, suffered a decline with some very rough going along the way. In 1675 there had been an insurrection against the excessive taxes (the "revolt of the stamped paper") imposed by Louis XIV. This uprising was cruelly suppressed by the governor of the province, Charles, duke of Chaulnes. The *parlement* was moved over to Vannes, and the inhabitants were subjected to a dreadful beating up, both physical and economic. Great numbers of them were ruthlessly killed, and the rest had their spirits crushed by heavy fines. In 1720 a great fire pretty well completed the ruin of the town. Some eight hundred houses were destroyed. It was into an environment that had been severely chastened that Robinet was born.

Early in life Robinet came under the tutelage of the Society of Jesus. Here again he fell upon troubled times which had a most important influence in determining the further course of his life. For a long time the Society had been gaining ground in France, and with the accession of Louis XIV its ascendency was obvious and admitted. Jesuit priests were trusted advisers of the king in matters of policy, and even went so far as to take his side in his quarrels with the pope. They forced the revocation of the edict of Nantes in 1685. What they

[1] Osborn, H. F. From the Greeks to Darwin. New York, 1894, pp. 121–123.

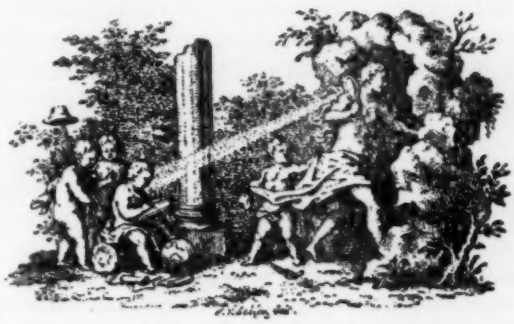
did to the Jansenists, culminating in the destruction and desecration of Port Royal in 1710, was the complete demonstration of their political power. But trouble was impending. The Society was too rich, too powerful, and too arrogant. Against the provisions of their own constitution, and the specific orders of many popes they were engaging in highly dubious as well as profitable commercial transactions on a large scale. Neither the lay nor the non-Jesuit ecclesiastical tycoons of the time approved, and more and more the Jesuits got a bad name with the common people. Nothing much happened overtly, however, until in 1741. In that year Benedict XIV issued a *breve*, in which he called these Jesuit *entrepreneurs* "disobedient, contumacious, captious and reprobate persons," and enacted a lot of new and more stringent laws for their better control. With various ups and downs matters went from bad to worse until in November, 1764 the Jesuits were suppressed in France by edict, being allowed to act only as secular priests. Finally in 1767 they were expelled from the kingdom. It was a bad time to be a Jesuit in France.

But some time before 1761 Robinet "*rentra dans le monde*" to devote himself to a literary life, and in that year, at the age of twenty-six, published in Amsterdam his first book, entitled *De la Nature*. It created a considerable stir. Various theologians of considerable standing attempted to refute his system, which was indeed a bizarre one. Robinet began as an animist, and always remained one. Everything in the universe has the power of reproduction; stones, stars, and all the rest. Furthermore, his whole philosophy was built around the idea of an unbroken evolutionary process, which again included the inorganic and the organic in one single series ending finally in man, as its highest product. Since he regarded the evolutionary process which produced all things from the primitive, original archetype to be due to natural, though unknown, causes, very little was left for God to do in the general scheme of things.

Robinet's first book had a great popular success. It is to this day one of the most curious books ever written on natural history. It went through a number of editions. In 1768 he published a shorter treatise specifically devoted to the exposition of his views on evolution, the title page of which is reproduced here in facsimile, and from which the first chapter is quoted below. Before going on to discuss this book, however, it may be noted that for many years Robinet led a precarious life as a teacher of English, translator, literary hack, dictionary writer, and editor. In the last capacity he was for a time

V U E
PHILOSOPHIQUE
DE LA
GRADATION NATURELLE
DES FORMES DE L'ETRE;
OU
LES ESSAIS
DE LA
N A T U R E
QUI APPREND A FAIRE L'HOMME.

PAR J. B. ROBINET.



A AMSTERDAM,
Chez E. VAN HARREVELT.
MDCCLXVIII.

associated with Benjamin Franklin and others in the production of a periodical of sorts called *Affaires de l'Angleterre et de l'Amerique*. For a time he lived in Bouillon (in Luxembourg) but in 1778 returned to Paris. Here his fortunes improved; he became a *censeur royal* and secretary to the minister Amelot. Finally he retired to Rennes, gave up writing, and sought to be forgotten. Two months before his death he made public retraction of the heretical principles he had professed, was gathered once more to the bosom of the church, and died in the odor of sanctity.

It is not easy to appraise Robinet justly. That he was a man of unusual talent, of wide learning of the scholastic sort, and of great productive energy admits of no doubt. His worst defect appears to have been most probably a direct result of his early training. He was enormously interested in natural phenomena, he wrote about nature, but he was not a competent naturalist. He sought the evidential basis for his philosophical speculations in books rather than in the field. His naive credulity, and nearly total lack of critical judgment as to the validity of evidence, are almost unparalled. Mermaids and mermen he accepts with perfect faith, pictures in beautiful copper plates, and assigns to an appropriate place in the evolutionary succession. In fact, the present writer knows of no other so complete a catalogue, with original descriptions quoted in full, of "authentic" accounts of these wonders as Robinet furnishes.

For quotation the first chapter of the *Vue philosophique* has been chosen,* as best giving a brief general view of Robinet's philosophy.

ALL BEINGS HAVE BEEN CONCEIVED AND FORMED AFTER A SINGLE DESIGN OF WHICH THEY ARE INFINITELY GRADUATED MODIFICATIONS. CONCERNING THIS PROTOTYPE AND ITS CHANGES, CONSIDERED AS SO MUCH PROGRESS TOWARDS THE MOST EXCELLENT FORM OF BEING, WHICH IS THE HUMAN FORM.

Since the progress of Nature is made by degrees which are often imperceptible and always by the smallest possible gradations, all its productions are as closely as possible related among themselves; although the sum of the differences accumulated along the universal scale of Beings can occasion doubt and uncertainty concerning the relation of the highest to the lowest. Each has its existence apart, and yet none is isolated or independent. Each one has resemblances more or less close to all the others, and even the extremes are related. They progress from one to another in a manner so close and so necessary that each finds the sufficient

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* I am indebted to Miss Hermine Grimm and Dr. Lawrence J. Henderson for aid and criticism in the preparation of the translation. The attempt has been to keep the translation as nearly literal as possible.

reason for its existence in that which precedes it, just as it is itself the sufficient reason for the existence of that which follows it.

Permit me here to call to mind this chain of all Beings, sufficiently established and developed elsewhere, which makes of all Nature a whole, continuous in varied beings; in which the imperfection of our knowledge makes us see interruptions and gaps, although there are really none at all and there cannot be any.

Nature is only a single act. This act comprises past, present, and future changes; its permanence constitutes the duration of things.

When I contemplate the innumerable multitude of individual things scattered over the surface of the earth, in its bowels, and in its atmosphere; when I compare the stone with the plant, the plant with the insect, the insect with the reptile, the reptile with the quadruped; I perceive, in spite of the differences which characterize each one of them, analogous resemblances which persuade me that they have been conceived and formed after a single plan, of which they are infinitely gradual variations. They all present the striking characteristics of this model—this original pattern—this prototype—which in realising itself, has assumed those successively infinitely multiplied and differentiated forms, under which Being manifests itself before our eyes.

These characteristics have by no means escaped observing minds; and if I undertake today to place in a clearer light a thought that others before me have had—contented to give some new degrees of certainty, according to my feeble capacity, to that which has been proposed rather as a conjecture or a doubt, not as a truth—I make a point of referring the honor of the discovery to those to whom it belongs.

At the summit of this grand scale of the inhabitants of the earth appears man, the most perfect of all. He unites, not indeed all the qualities of the others but all that are compatible in a single, same species, raised to a higher degree of perfection. He is the masterpiece of Nature which the gradual progression of Beings was, or was destined to have for a final limit; at least we here take it to be the final one, because it is with man that our natural scale of Beings ends.

In the wonderfully varied series of animals inferior to man, I see Nature striving to advance, feeling her way toward this excellent Being which crowns her work. However imperceptible may be the progress which she has made at each step (that is to say, at each new production—at each achieved variation of the primitive design), the progress becomes very evident after a certain number of such changes. If, for example, the gradation between two related quadrupeds, such as the horse and the zebra, is too delicate for us to judge which of the two approaches a man more closely than the other, in the scale; nevertheless the Zoologist who passes from bipeds to *bimanes*, then to quadrupeds (solipeds, cloven footed animals, *fissipedes*) and from those to *quadrumanes*, perceives that he mounts by degrees to the summit of the scale where he finds the only animal which is at the same time bimanual and bipedal. Coming finally to compare these different animals, he recognizes without difficulty that a *quadrumane* such as the *magot* or Orang-outang resembles man much more than does any quadruped including even a *fissipede;* and in turn man resembles a *solipede* much less than a *fissipede*, and particularly less than those *fissipedes* which use their fore-feet as hands. However little our Zoologist wishes to give attention to all possible comparable characteristics, he

still will discover that the Orang-outang resembles man more than any other animal.

As many as there are intermediate variations from the prototype to man so many do I count the trials of Nature which, aiming at the most perfect, can only attain it, however, by this countless series of rough sketches. For natural perfection consists in unity combined with the greatest variety possible. Accordingly it is the extreme of the variation of the original form which gives the most perfect form; and, this extreme which terminates the series of intermediate variations needs the experience of these in order to attain this last condition.

Nature could only realize the human form by combining in every imaginable manner each of the traits which were to enter it. If she had overlooked a single combination there would not have been this precise degree of fitness, which has been acquired in passing through all the gradations.

From this point of view, I picture each variation of the morphology of the prototype, as a study of the human form which Nature plans, and I believe the collection of these studies can be called the apprenticeship of Nature, or the trials by which Nature learns to make man.*

What I say concerning man in his relation to all the other Beings, is perhaps equally applicable to any term whatever of the scale relative to those which precede it. But man being that which we know as the most excellent thing on our planet, we should not have any idea at all of the full richness of Nature, if we contented ourselves in viewing her in any single lower organism.

When one studies the human machine, this vast multitude of systems combined into a single one—this enormous aggregation of parts, energy, forces, similarities, motions, the number of which overwhelms the mind—although one has only an infinitesimal knowledge about it all, one is not astonished that there has been necessary such a long succession of arrangements and rearrangements, compositions and dissolutions, additions and suppressions, alterations, obliterations, transformations of all kinds, to produce an organization so skillful and so marvelous.

But what eye would be sufficiently keen to recognize the rough draught of man, not only in the first realization of the prototype, which is beyond the reach of our senses and which we can only imagine, but even in the least of the forms that we can perceive, so far from the prototype, and for that very reason so much nearer man? Who will be able to follow this sketch in its entire development? Who will be able to do violence to Nature, to tear away her secret, to show her unceasingly improving her work, adding faculties to faculties, organs to organs; varying these organs in all the ways they are capable of being varied, sometimes lengthening them and again contracting them, burying them in one individual while developing them in another, suppressing some in order to reproduce them afterwards with a new apparatus; in a word, making man in detail and by fragments, working and multiplying each piece indefinitely without ever copying, in order to compose of them an infinity of different Beings; instilling into each Being her inexhaustible fecundity so as to form from it what taxonomists call a species, lasting monument of the gradation of its progress; and finally by these generating processes obtaining the masterpiece she has planned.

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* Pliny has called the bind weed, a small flower greatly resembling the lily, the experiment of Nature in trying to make a lily, *Convolvulus tyrocinium Naturae lilium formare discentis.*

Man (I mean man taken in a general and abstract sense as the model of the species) is the prototype, plus the results of all the combinations that the prototype has undergone in passing by all the limits of the universal progression of the Being. If some genius, well enough acquainted with the progression of Nature to recognize what the prototype has acquired at each step, could deprive man of the acquisitions in the same descending progression and with the same inversion of changes, he could make him go back toward the bottom of the scale where he would be reduced to the simple primitive form of the prototype. But from the first step of his degradation he would have to cease being a man, for the creature nearest to man is almost a man; but yet is not one.

Since man is the prototype plus the result of all the combinations which the prototype has undergone in passing through all the degrees of development, why should not the prototype be man, less this same result; for the result is precisely the difference which constitutes man? To say that the prototype is man less this result, is to say that the prototype is man, less that which makes man. And what does such an expression mean, except that the prototype is not man?

The prototype is a model which represents the Being reduced to the lowest terms; it is an inexhaustible source of variations. Each variation when realized gives a Being, and may be called a metamorphosis of the prototype, or rather its first envelope which had been the first realization of it. The prototype is an intellectual principle which only alters itself in realizing itself in matter.

A cavern, a grotto, a savage's hut, a shepherd's cabin, a house, a palace, can be considered as gradual variations of the same plan of architecture which begins to be executed with the fewest elements possible. A savage's hut, a shepherd's cabin, are not at all an Escorial or a Louvre; but they may be regarded as types more or less distant, in that the former like the latter have a resemblance to the same primitive design, and that they are all the product of the same idea, more or less developed. One finds in the most wretched cabin the same essential rooms as in the most magnificent palace. The whole difference between the cabin and the palace consists in the number of rooms, their construction, proportion, arrangement, ornamentation, all things which are derived from the original plan, so to speak, by means of evolution. Not only all the buildings of man, although so varied in the same nation, and yet more dissimilar among different nations, go back to the same plan; but this plan furthermore includes all the crude habitations that the animals know how to make according to their needs and instincts.

A stone, an oak, a horse, a monkey, a man, are gradual variations of the prototype which has begun to realize itself by the fewest elements possible. A stone, an oak, a horse are not at all men; but they can be regarded as more or less crude types of man, in that they go back to the same primitive design, and that they are all the product of the same idea, more or less developed. One finds in the stone and in the plant, the same principles essential to life as in the human machine; the only difference consists in the combination of these principles, the number, proportion, order, and form of the organs.

Considering the progression of individuals, whatever they are called, as so much progress of the being towards humanity, we shall compare them first with the exterior and interior human form, or physical man, then with the faculties of a higher order, that is to say, man endowed with a mind.

This new manner of contemplating Nature and her production which refers them all to a single universal generating idea, is founded on the principle of continuity which links together all the parts of this great whole. Each mechanism, taken in particular, tends properly and immediately to produce only that which in reality it engenders, but the sum of these mechanisms tends to the final result. And we here take man as the final result, in order to limit ourselves to terrestrial Beings, the only ones within our reach.

In conclusion it may be said that the *Vue philosophique* will furnish an hour's entertainment for any biologist, by its pictures if for no other reason. On Plate IX is figured a fully developed foetus, which Robinet says he received in 1762 from the coast of Angola, and states to be "*un foetus-pongo.*" Dr. Adolph H. Schultz kindly permits me to state that, in his opinion, the foetus figured was certainly human, and not that of an orang.

RAYMOND PEARL.

THE HISTORY OF A GESTURE

The following quotation is taken from a delightful but now little known book by David Urquhart, entitled "*The Pillars of Hercules; or, a Narrative of Travels in Spain and Morocco in* 1848," published, in two volumes, by Richard Bentley, in London, in 1850. David Urquhart was a brilliant man who had a not altogether happy career as a diplomat, and was later a member of Parliament. A minor claim to fame was that he promoted the use of the Turkish bath in England. Another interest was found in the Phoenicians, about whom he had a considerable and respectable store of learning. The quotation is from the first volume (pp. 199–203). The latter part of the passage gives the most succinct account known to me of what happened to ancient literature, in the course of what we proudly call the "progress of civilization."

"I was looking over some coins which a gentleman at Gibraltar had collected, and was astounded to come upon one which is not copied, but which is represented in the accompanying wood-cut.* This told the whole story of the glass-houses and the tin. I wonder if the coin was censured as indiscreet at Tyre. How is it

* The coin is in one of the addenda to Flores;—it is not in the copy at the British Museum. The coin is, however, known in the medal room.

that by putting the hand in this fashion to the nose the fancy should be tickled? Whence did the custom come? How did it travel to Britain? One is not prepared to have to search for such a gesture in the Hebrew Talmudists, or the Greek scholiasts; but here it is raised to numismatic dignity, and is worthy of the philosopher.

There is a ludicrously supercilious animal, very strong and very stupid, with a horn on his nose, belonging to Africa, the Holy Land, Mesopotamia—in fact, all the Phœnician countries. He was the Behemoth, for of no other animal could Job be thinking when he said, "With his nose he pierceth through snares"—the horn, emblem of victorious strength, denoting by its exaltation its own achievements, and the proud bearing of the brow on which it is planted. Each year gives to it increase, and each increase is marked by a wrinkle which comes to signify acquirement. There are false acquirements as there are true; and the horn of the nose is the burlesque of the horn of the forehead. The motion that is given to the hand shows that it is the spiral wreathings of a horn that are imitated: the rhinoceros represents the one, the unicorn the other.

Of the two images, the African has preserved the grave one, we the grotesque. The Abyssinian warrior, when he has gained a victory, adorns his forehead with a horn. The London coalheaver, when he has made a hit, puts his thumb to his nose.

This gesture in its grotesque form was known not long ago in Spain, although at present it appears to have died out. Cervantes unmistakably describes it, and in the person of Sancho Panza; the English, have therefore the sole honour and distinction of preserving this peculiarity of the Phœnicians and Etruscans.*

I might be inclined to place beside this, the group of lions and unicorns at Persepolis, which so closely resemble the supporters of the English arms, as scarcely to be referable to coincidence. They are, indeed, of recent adoption as the arms of England, but of ancient date in those of Scotland. The emblematic plants of England were, however, those of Phœnicia—the oak and the ivy; and the rose of England is still the flower of Spain. The blood-red hand of Ulster is in Morocco stuck above every door. It wants not so much to raise the thought, or justify the association. Instinctively one seeks for some sympathetic deed, which shall link us to the Phœnicians; and Spain lies between, and is bound therewith: she too at length prides herself on her Moorish blood, and exalts herself (or at least did so till we robbed her fortress) on her British friendship recorded in the proverb:

Guerra con toda la tierra,
Pero par con Ynglaterra.

The extinction of written records has given importance in these countries to every trifling usage or tradition, as will be best felt by reviewing the catalogue of mischances which have befallen the literature of Africa, and of the great people, who in the West have given to it its celebrity.

* It is figured on a vase in the Museo Borbonico.

Alexander destroyed the libraries of Tyre: those of Sidon perished in the flames with their wealth and themselves. The whole mass of the literature of Carthage was destroyed by the Romans, except a small portion given to Massinissa.

The Alexandrian library was burnt by the troops of Julius Caesar. The various collections made at Rome by Asinius Pollio, Augustus, and Tiberius, were lost in the fires under Nero and Titus. Domitian endeavoured to repair the disaster by getting the manuscripts of private collections copied, and ransacking Africa for the lost works: these were deposited in the Temple of Peace, and destroyed by fire under Commodus.

Finally, the gleanings of Rome were carried off by Genseric and *lost at sea*. The persecution of the Donatists led to the burning, all over Africa, of books and manuscripts. The Mussulman conquests led to fresh burnings, and the great African collections of Alexandria again perished under Omar.

The 600,000 volumes of Cordova, and the enormous collections of the learned cities of the Moors, perished by Christian and Gothic hands. The library of Tunis was destroyed by Charles V; Muley Hassan lamented it more than his city. After the ravages of war had ceased, Cardinal Ximenes, the munificent patron of literature, consigned to flames 88,000 African manuscripts. Lastly came the capture of the library of the King of Morocco, a portion of which constitutes the collection of the Escurial, and this again has suffered by fire.

Thus have been swept away the literary records of this quarter of the globe, as completely as devouring sands and the human ravages of more recent times have effaced all local signs. The curiosity of the traveller is arrested on its inhospitable shores; the research of the antiquarian baffled by the scantiness or uncertainty of data. Her history remains what her interior still is: we can wander, guided only by the stars—little points of light that shine only because of the surrounding darkness.

R. P.

RECENT LITERATURE

USEFUL IN THE STUDY OF HUMAN BIOLOGY

In this department will be noticed the books and reprints which are currently received at the Editorial Office. Publications intended for notice here should be addressed to Dr. Raymond Pearl, Department of Biology, 1901 East Madison Street, Baltimore, Maryland, U. S. A.

I. EVOLUTION AND GENETICS

BLACHLY, CHARLES D. The mechanics of heredity. (2 articles.) *Oklahoma City* (Charles D. Blachly, 408 Medical Arts Bldg.), 1929, 18 pp.; 1930, 14 pp.

BLACK, DAVIDSON. Interim report on the skull of *Sinanthropus*. *Bulletin of the Geological Society of China*, Vol. IX, No. 1, pp. 7–23, 1930.

BRAMBELL, F. W. ROGERS (with a Preface by Julian S. Huxley). The Development of Sex in Vertebrates. *New York* (The Macmillan Co.), 1930. Pp. xvi + 261. $5\frac{3}{8} \times 8\frac{1}{2}$ inches. $4.00.

CASTLE, W. E. Genetics and Eugenics. A text-book for Students of Biology and a Reference Book for Animal and Plant Breeders. Fourth Revised Edition. *Cambridge* (Harvard University Press), 1930. Pp. x + 474. $5\frac{3}{4} \times 9$ inches. $3.00.

COHEN-KYSPER, ADOLF. Das Determinationsproblem in analytischer Darstellung. *Berlin* (Julius Springer), 1930. Pp. 48. $6\frac{1}{2} \times 9\frac{1}{2}$ inches. 4.60 marks (paper).

CREW, F. A. E. Whither and how? *Scientific Agriculture*, Vol. IX, No. 12, pp. 765–772, 1929.

CURTIUS, FRIEDRICH. Familiäre diffuse Sklerose und familiäre spastische Spinalparalyse in einer Sippe. Ein Beitrag zur Genealogie der Heredodegenerationen. *Zeitschrift für die gesamte Neurologie und Psychiatrie*, Band 126, Heft 1 und 2, pp. 209–227, 1930.

CURTIUS, FRIEDRICH. Nachgeburtsbefunde bei Zwillingen und Ähnlichkeitsdiagnose. *Archiv für Gynäkologie*, Band 140, Heft 2 and 3, pp. 361–366, 1930.

DAVENPORT, CHARLES B. Are there genetically based mental differences between the races? *Science*, Vol. LXVIII, No. 1773, p. 268, 1928.

DAVENPORT, CHARLES B. Heredity and longevity. *Proceedings of the Third Race Betterment Conference*, Jan. 2–6, 1928, pp. 15–18.

DAVENPORT, CHARLES B. Light thrown by genetics on evolution and development. *Scientific Monthly*, Vol. XXX, pp. 307–314, 1930.

FISCHER, EUGEN. Aus der Geschichte der Deutschen Gesellschaft für Rassenhygiene. *Archiv für Rassen- und Gesellschaftsbiologie*, Bd. XXIV, 5 pp., 1930.

FISCHER, EUGEN. Erbschädigung beim Menschen. *Das Kommende Geschlecht*, Band V, Heft 6, 27 pp., 1930.

FISCHER, EUGEN. Europäer-Polynesier-Kreuzung. *Zeitschrift für Morphologie und Anthropologie*, Bd. XXVIII, Heft 1, pp. 205–209, 1930.

FISCHER, R. A. Biometry and evolution. *Nature*, August 16, 1930. 4 pp.

GUYÉNOT, ÉMILE. La Variation et l'Évolution. Tome II. L'Évolution. *Paris* (G. Doin et Cie), 1930. Pp. viii + 414. 4¼ × 7 inches. 32 francs (paper).

HIRSCH, NATHANIEL D. MTTRON. Twins. Heredity and Environment. *Cambridge* (Harvard University Press), 1930. Pp. 159. 5½ × 8¾ inches. $2.00.

HOLMES, S. J. Factorial concept of heredity in relation to disease, especially skin disease. *Archives of Dermatology and Syphilology*, Vol. 21, April, pp. 558–565, 1930.

HOLMES, S. J. Natural selection in man and the evolution of human intelligence. (The Galton Lecture, 1930.) *Eugenics Review*, Vol. XXII, No. 1, pp. 7–16. 1930.

JUDD, NEIL MERTON. The excavations and repair of Betatakin. *Proceedings of the United States National Museum*, Vol. 77, Art. 5, pp. 1–77, pls. 1–46, 1930.

LUQUET, G. H. The Art and Religion of Fossil Man. Translated by J. Townsend Russell, Jr. *New Haven* (Yale University Press), 1930. Pp. xiv + 213. 6¼ × 10 inches. $5.00.

MEEK, ALEXANDER. The Progress of Life. A Study in Psychogenetic Evolution. *New York* (Longmans, Green and Co.), 1930. Pp. vii + 193. 5½ × 8½ inches. $4.20.

MICHON, PAUL. Les Groupes Sanguins. Schémas et Applications Pratiques. La Transfusion Sanguine. Technique et Indications. *Paris* (Masson et Cie), 1930. Pp. 120. 5½ × 7⅞ inches. 16 francs (paper).

OSBORN, HENRY F. Paleontology versus genetics. *Science*, Vol. LXXII, No. 1853, pp. 1–3. 1930.

PEARL, RAYMOND. Requirements of a proof that natural selection has altered a race. *Scientia*, Vol. XLVII, pp. 175–186, 1930.

PLATE, L. Einige Bemerkungen zu dem Aufsatz von E. Study über "Neuere Angriffe auf die Selektionstheorie," *Archiv für Rassen- und Gesellschaftsbiologie*, Band 22, Heft 4, pp. 457–460, 1930.

PLATE, L. Review of "Kohts, N., Adaptive motor habits of the *Macacus rhesus* under experimental conditions." Moskau, 1928. *Archiv für Rassen- und Gesellschaftsbiologie*, Band 22, Heft 1, pp. 69–70, 1929.

RIGNANO, EUGENIO. The Nature of Life. *New York* (Harcourt, Brace and Co.), 1930. Pp. x + 168. 5½ × 8½ inches. $2.75.

UTTERBACK, WILLIAM I. Is both special and evolved creation possible? An illustrated presentation based on science and scripture. *Chi Beta Phi Record*, Vol. IV, May, 1930. 20 cents.

VERNE, JEAN. Couleurs et Pigments des Êtres Vivants. *Paris* (Armand Colin), 1930. Pp. 219. 4⅜ × 6¾ inches. 10.50 francs (paper), 12 francs (bound).

VON VERSCHUER, OTMAR. Erbuntersuchungen an tuberkulösen Zwillingen. Berliner medizinische Gesellschaft. Sitzung vom 18. Juni, 1930. *Medizinische Klinik*, Jahrg. 1930, Nr. 27. 4 pp.

VON VERSCHUER, OTMAR. Intellektuelle Entwicklung und Vererbung. Pp. 176–207 of "Vererbung and Erziehung," edited by Günther Just, and published by Julius Springer, *Berlin*. 12.80 marks (paper); 14.60 marks (cloth), 1930.

VON VERSCHUER, OTMAR. Soziale Umwelt und Vererbung. *Ergebnisse der Sozialen Hygiene und Gesundheitsfürsorge*, Band II, pp. 1–33, 1930.

VON VERSCHUER, OTMAR. Vom Umfang der erblichen Belastung im deutschen Volke. *Archiv für Rassen- und Gesellschaftsbiologie*, Bd. XXIV, pp. 238–268, 1930.

WEINERT, HANS. Menschen der Vorzeit. Ein Überblick über die altsteinzeitlichen Menschenreste. *Stuttgart* (Ferdinand Enke Verlag), 1930. Pp. 139. 6½ × 10 inches. 8 marks (unbound); 9.50 marks (bound).

WELLS, H. GIDEON. The evidence furnished by biochemistry and immunology on biologic evolution. *Archives of Pathology*, Vol. 9, pp. 1044–1075, 1930.

II. GROUP BIOLOGY

BERCZELLER, L. Zur Statistik der Wissenschaften und Gelehrten. 6 mimeographed pages. Mimeographed by Dr. Fritz Löw, Wien 8, Josefstädterstrasse 35. No date.

BOWER, JOHN O. Acute appendicitis in Philadelphia. *Monthly Bulletin of the Department of Public Health of the City of Philadelphia*, September, 1930. 16 pp.

BOWMAN, ISAIAH. Antarctica. *Proceedings of the American Philosophical Society*, Vol. LXIX, pp. 19–43, 1930.

BUREAU OF THE CENSUS. Religious Bodies: 1926. Volume I. Summary and Detailed Tables. *Washington* (U. S. Government Printing Office), 1930. Pp. vi + 769. 5¾ × 9 inches. $1.50.

BURGDÖRFER, FRIEDRICH. Bevölkerungsfrage und Steuerreform. *Das Kommende Geschlecht*, Band V, Heft 4/5, 68 pp., 1930.

BURGDÖRFER, FRIEDRICH. Bevölkerungszunahme und Geburtenbeschränkung in Deutschland. *Zentralbl. f. d. ges. Hygiene*, Bd. 22, Heft 11, pp. 657–669, 1930.

BURGDÖRFER, FRIEDRICH. Die bevölkerungspolitische Lage und das Gebot der Stunde. *Archiv für Rassen- und Gesellschaftsbiologie*, Band 23, Heft 3/4, pp. 166–230, 1930.

BURGDÖRFER, FRIEDRICH. Familie und Volk. *Reichsbund der Kinderreichen Deutschlands zum Schutze der Familie e. V.* (Lützowstrasse 75, Berlin, W. 35). 70 pp., 1930.

BURGDÖRFER, FRIEDRICH. Die neuen deutschen Sterbetafeln und ihre Lehren. *Ergebnisse der Sozialen Hygiene und Gesundheitsfürsorge*, Bd. II, pp. 298–359, 1930.

CAISSE NATIONALE DES ASSURANCES SOCIALES. L'Assurance Obligatoire contre la Tuberculose. Loi et Règlement. *Roma* ("L'Universale" Tipografia Poliglotta), 1930. 24 pp.

COLLINS, SELWYN D. Influenza-pneumonia mortality in a group of about 95 cities in the United States 1920–1929. Reprint No. 1355 from the *Public Health Reports*, Vol. 45, No. 8, pp. 361–406, 1930. *Washington* (U. S. Government Printing Office). 10 cents (paper).

COMMISSIONER OF INTERNAL REVENUE, STATISTICAL SECTION, INCOME TAX UNIT. Statistics of income for 1928 (preliminary report). Compiled from income tax returns for 1928, filed to August 31, 1929. *Washington* (U. S. Govern-

ment Printing Office), 1930. Pp. iii + 20. 5¾ × 9⅛ inches. 5 cents (paper).

COMPTON, RALPH THEODORE. Fiscal problems of rural decline. A study of the methods of financing the costs of government in the economically decadent rural areas of New York state. *State of New York. Special Report of the State Tax Commission.* No. 2., 283 pp., 1929.

CRUM, ROY W.; Edited by. Proceedings of the Ninth Annual Meeting of the Highway Board Held at Washington, D. C. December 12–13, 1929. *Washington* (National Research Council), 1930. Pp. 403. 6¾ × 9¾ inches. $2.00 (paper).

DEPORTE, J. V. Heart disease in the state of New York. A statistical review of mortality and morbidity. *The American Heart Journal*, Vol. V, No. 5, p. 652, 1930.

DEPORTE, J. V. Some aspects of the recorded and resident mortality from tuberculosis in New York State in 1927 and 1928. *American Review of Tuberculosis*, Vol. XXII, No. 1, pp. 87–115, 1930.

EDIN, KARL A. Födelsekontrollens inträngande hos de breda lagren. *Ekonomisk Tidskrift*, pp. 123–152, 1929.

EMERSON, WILLIAM R. P. The Diagnosis of Health. *New York* (D. Appleton and Co.), 1930. Pp. xiv + 272. 5¼ × 8 inches. $3.00.

FAIRCHILD, DAVID. Exploring for Plants. From Notes of the Allison Vincent Armour Expeditions for the United States Department of Agriculture, 1925-1926, and 1927. *New York* (The Macmillan Co.), 1930. Pp. xx + 591. 6 × 9¼ inches. $5.00.

GAFAFER, WILLIAM M. Infant diarrhea and enteritis and climate. London, 1876–1927. *American Journal of Hygiene*, Vol. XI, No. 3, pp. 535–575, 1930.

GEARY, R. C. The mortality from tuberculosis in Saorstat Eireann. A statistical study. *Journal of the Statistical and Social Inquiry Society of Ireland*, pp. 67–103, 1930.

GINI, CORRADO, SHIROSHI NASU, ROBERT R. KUCZYNSKI and OLIVER E. BAKER. Population. Lectures on the Harris Foundation 1929. *Chicago* (University of Chicago Press), 1930. Pp. ix + 312. 5 × 7½ inches. $3.00.

GREENWOOD, M. The vaccination problem. *Journal of the Royal Statistical Society*, Vol. XCIII, Part II, pp. 233–270, 1930.

GREULICH, WILLIAM W. The incidence of human multiple births. *American Naturalist*, Vol. LXIV, pp. 142–153, 1930.

HANKINS, FRANK H. Does advancing civilization involve a decline in natural fertility? *Publication of the American Sociological Society*, Vol. XXIV, No. 2, pp. 115–122, 1930.

HARPER, ROLAND M. Alabama scientists. *Alabama Alumni News, University, Alabama*, Vol. 12, no. 2, 1928. 3 pp.

HARPER, ROLAND M. Alabamians in "Who's Who." *Montgomery Advertiser*, Vol. 99, no. 359, Dec. 25, 1927. 4 pp.

HARPER, ROLAND M. Changes in rate of population growth in certain southeastern states. *Reprint of letter published under the title "Shift in Population,"* in Current History (New York), Vol. 32, June, 1930. Pp. 620–621.

HARPER, ROLAND M. Divorce and crime on Prince Edward Island. *Eugenics*, Vol. III, No. 5, pp. 182–189. 1930.

HARPER, ROLAND M. Florida scientists. *The Florida Naturalist*, Vol. III, No. 4, pp. 98–99, 1930.

HARPER, ROLAND M. Georgia scientists. *Georgia Alumni Record*, June, 1928. 2 pp.

HARPER, ROLAND M. The increasing span of life in Athens. *Annual Report of the Department of Health, City of Athens and Clarke County, Georgia, for* 1929. Pp. 24–26. 1930.

HARPER, ROLAND M. Matrimonial prospects of southern college women. *Journal of Heredity*, Vol. XXI, No. 1, pp. 29–33, 1930.

HARPER, ROLAND M. North Carolina scientists. *The Journal of the Elisha Mitchell Scientific Society*, Vol. 45, No. 2, pp. 333–337, 1930.

HARPER, ROLAND M. Quality of population in Kent and Ravenna, Ohio. *Published with the title "Giving Us the Acid Test," in the Letters to the Editor column of the Akron (Ohio) Beacon-Journal*, Vol. 91, No. 172, p. 4, June 24, 1930.

HARPER, ROLAND M. Religion and family size in Tallahassee, Florida. *Journal of Heredity*, Vol. XIX, No. 4, pp. 169–173, 1928.

HARPER, ROLAND M. A simple measure of fecundity based on census figures. *Journal of Heredity*, Vol. XVIII, No. 5, pp. 217–223, 1927.

HARPER, ROLAND M. Some neglected aspects of the immigration problem. Statistical comparisons of native and foreign whites, and natives of foreign and mixed parentage, in the United States. *Eugenics*, Vol. II, No. 10, pp. 24–34 and No. 11, pp. 22–30. 1929.

HARPER, ROLAND M. A statistical study of the Mount Pleasant Cemetery. *River Junction (Fla.) Tribune*, June 20, 1930. 2 pp.

HEDRICH, A. W. The corrected average attack rate from measles among city children. *American Journal of Hygiene*, Vol. XI, No. 3, pp. 576–600, 1930.

HIMES, NORMAN E. Contraceptive methods: the types recommended by nine British Birth Control Clinics. *New England Journal of Medicine*, Vol. 202, pp. 866–873, 1930.

HOFFMAN, FREDERICK L. Malaria in Haiti. *Southern Medical Journal*, Vol. XXIII, No. 5, pp. 459–461, 1930.

HOLLAND-RANTOS CO., INC. Suggestions for contraceptive practice. For distribution to members of the medical profession only. (156 Fifth Ave., New York.) 12 pp., 1930. May be secured by physicians free of charge.

HOLMES, S. J. The biological trend of the negro. *University of California Chronicle*, January, 1930, pp. 38–70.

HUNTINGTON, ELLSWORTH. Weather and Health. A Study of Daily Mortality in New York City. Prepared under the direction and with the advice of the Committee on the Atmosphere and Man, Division of Biology and Agriculture, National Research Council. Bulletin of the National Research Council No. 75. *Washington* (National Research Council), 1930. Pp. 161. 6¾ × 9¾ inches. $2.00 (paper).

HOFFMAN, FREDERICK L. The suicide record for 1929. *The Spectator*, May 22, 1930, 4 pp.

JOHNSON, CHARLES S. The Negro in American Civilization. A Study of Negro Life and Race Relations in the Light of Social Research. *New York* (Henry Holt and Co.), 1930. Pp. xiv + 538. 5½ × 8½ inches. $4.00.

KOPF, EDWIN W. Glossary and bibliography section of the paper entitled "Notes on the Origin and Development of Reinsurance." *Proceedings of the Casualty Actuarial Society*, Vol. XVI, Part I, pp. 76–92, 1929.

LANE-CLAYPON JANET E. Report on cancer of the lip, tongue and skin. An analysis of the literature, from a statistical standpoint, with special reference to the results of treatment. Reports on Public Health and Medical Subjects No. 59. *London* (His Majesty's Stationery Office), 1930. Pp. 121. 6 × 9½ inches. 2 shillings.

LAUGHLIN, HARRY H. The legal status of eugenical sterilization. History and analysis of litigation under the Virginia Sterilization Statute, which led to a decision of the Supreme Court of the United States upholding the statute. *Supplement to the Annual Report of the Municipal Court of Chicago* for the year 1929. 83 pp.

LOCKEMANN, GEORG. Geschwisterzahl und Lebensleistung. (Bedeutende Männer aus kinderreichen Familien.) *Archiv für Sippenforschung und alle verwandten Gebiete*, Heft 4, 1930. 6 pp.

LONDON MINISTRY OF HEALTH. Memorandum on cancer as a subject for the attention of local authorities. Circular 1136, 10 pp. 1930.

MATTHEWS, ELLEN N. Children in Fruit and Vegetable Canneries. A Survey in Seven States. United States Department of Labor, Children's Bureau Publication No. 198. *Washington* (U. S. Government Printing Office), 1930. Pp. vii + 227. 5¾ × 9⅛ inches. 40 cents (paper).

MCKENZIE, MARY. Late results of the Manitoba epidemic of poliomyelitis of 1928. *Canadian Medical Association Journal*, Vol. XXI, pp. 219–294, 1929.

MUCKERMANN, HERMANN. Differenzierte Fortpflanzung. *Archiv für Rassen- und Gesellschaftsbiologie*, Bd. XXIV, pp. 269–290. 1930.

NAUKOWEGO INSTYTUTU. Kwartalnik Naukowego Instytutu Emigracyjnego Ikolonjalnego oraz Przeglad Emigracyjny (The Quarterly Review of the Scientific Institute for Emigration and Colonization). Tom I i II, 1930. 566 pp. *Warsaw, Poland.*

PEARL, RAYMOND. Alcohol. Biological aspects. *Encyclopaedia of the Social Sciences*, Vol. I, pp. 620–622, 1930.

PEARL, RAYMOND. Alkohol und Lebensdauer. *Hannover* (Norddeutsches Druck- und Verlagshaus), 1930. Pp. 263. 5½ × 8½ inches.

PEARL, RAYMOND. Statistical report on the second year's operations of the Bureau for Contraceptive Advice. *Second Report of the Bureau for Contraceptive Advice* (1028 No. Broadway, Baltimore), pp. 2–10, 1930.

PEEBLES, ALLON. A survey of the medical facilities of Shelby County, Indiana: 1929. Abstract of Publication No. 6. *The Committee on the Costs of Medical Care*, Washington, D. C., 20 pp., 1930.

ROBINSON, CAROLINE H. Seventy Birth Control Clinics. A Survey and Analysis Including the General Effects of Control on Size and Quality of Production. *Baltimore* (The Williams and Wilkins Co.), 1930. Pp. xx + 351. 5⅝ × 8⅜ inches. $4.00.

RÖSSLE, ROBERT. Die Besonderheiten der Sterblichkeit in Basel. Ein Beitrag zur geographischen Pathologie. *Verhandlungen der Naturforschenden Gesellschaft in Basel*, Band XL, 27 pp., 1929.

SEYBERT INSTITUTION. A Living Hand. Ten-year report of the Seybert Institution, 1920–1930. 311 *South Juniper St., Philadelphia.* 48 pp., 1930.

United Fruit Company Medical Department (17 Battery Place, New York). Eighteenth Annual Report, 1929. Pp. 451.

Walch, E. W., and C. J. Schuurman. Saltwater fishponds and malaria. *Mededeelingen van den Dienst der Volksgezondheid in Ned.-Indië*, 1929. 26 pp.

Wilbur, Ray Lyman. The first three years' work of the Committee on the Costs of Medical Care and its plans for the future. An address at the Committee's spring meeting, 1930. *Committee on the Costs of Medical Care*, 910 Seventeenth St., N.W., Washington. 10 pp., 1930.

Wolff, Georg. Bevölkerungsentwicklung und Sterblichkeit in Berlin. *Die Medizinische Welt.* Nr. 19/20. 15 pp. 1930.

Wolff, Georg. Calmettes Erfolgsstatistik. *Die Medizinische Welt*, Nr. 26. 6 pp., 1930.

Wolff, Georg. Über die Ursachen des Rückganges der Tuberculosesterblichkeit in den Kulturstaaten. *Zeitschrift für Tuberkulose*, 22 pp. 1930.

III. BEHAVIOR

Allen, C. N. Recent studies in sex differences. *Psychological Bulletin*, Vol. 27, pp. 394–407, 1930.

Association Against the Prohibition Amendment: England's solution of the liquor problem. *Association Against the Prohibition Amendment* (Washington, D. C.) September, 1930. 69 pp.

Association Against the Prohibition Amendment. Finland's prohibition: An Echo of Volsteadism. *Association Against the Prohibition Amendment* (National Press Building, Washington). 36 pp. June, 1930.

Bacon, Selden. How the Tenth Amendment affected the Fifth Article of the Constitution. *Virginia Law Review*, Vol. XVI, No. 8, 23 pp., 1930.

Barnes, Harry E. The Story of Punishment. A Record of Man's Inhumanity to Man. *Boston* (The Stratford Co.), 1930. Pp. vii + 292. 5½ × 8½ inches. $3.00.

Bean, Donald P. Universities and publishing. *Press Impressions*, March, 1930, 4 pp.

Bland-Sutton, Sir John. The Story of a Surgeon. *Boston and New York* (Houghton Mifflin Co.), 1930. Pp. xii + 204. 5½ × 8¾ inches. $3.50.

Briand, M. Memorandum on the organization of a regime of European Federal Union. Addressed to twenty-six governments of Europe by M. Briand, Foreign Minister of France, May 17, 1930. International Conciliation Special Bulletin. *Carnegie Endowment for International Peace*, 44 Portland St., Worcester, Mass. Pp. 325–353, June, 1930.

Browne, C. R. Maori Witchery. Native Life in New Zealand. *London and Toronto* (J. M. Dent and Sons, Ltd.), 1929. Pp. x + 210. 5⅛ × 7¼ inches. $2.00.

Comments on the Political and Economic Status of the Philippines. Foreword, by Almer M. New-Hall. American rights and interests in the Philippines, by David P. Barrows. Congress and Philippine independence, by Daniel R. Williams. *Issued by the San Francisco Chamber of Commerce.* 30 pp. Undated.

Commission on Medical Education. Medical education and related problems in Europe. *New Haven.* 200 pp. April, 1930.

DERCUM, FRANCIS X. On the nature of thought and its limitation. *Proceedings of the American Philosophical Society*, Vol. LXVIII, pp. 275–302, 1930.

DESCAMPS, PAUL. État Social des Peuples Sauvages. Chasseurs—Pêcheurs—Cueilleurs. *Paris* (Payot), 1930. Pp. 288. 5½ × 9 inches. 30 francs (paper).

EARP, J. ROSSLYN. Gametology: A plea for a new specialty. *Medical Journal and Record*, June 18, 1930. 3 pp.

THE FINAL SETTLEMENT OF THE REPARATIONS PROBLEMS GROWING OUT OF THE WORLD WAR. Protocol, with annexes, approved at the Plenary Session of The Hague Conference, August 31, 1929 and agreements concluded at The Hague Conference, January, 1930. *International Conciliation*, September, 1930, No. 262. 205 pp. Carnegie Endowment for International Peace, 44 Portland Street, Worcester, Mass.

GRUENBERG, BENJAMIN C. A study of indoctrination in science teaching. *Science Education*, May, 1930. 14 pp.

HARPER, ROLAND M. The social environment of anti-evolution legislators. *The Georgia Alumni Record*, Vol. 8, No. 7, April, 1929. 4 pp.

HOLMES, S. J. The freaks of creative fancy. *Psychological Review*, Vol. 36, No. 5, pp. 446–449. 1929.

HUGHES, PERCY. Forms of generalization and their causes. Cooperation or conflict in the study of learning? *Lehigh University Publication*, Vol. IV, No. 8, pp. 281–360. 1930. Lehigh University, Bethlehem, Pennsylvania. Price 10 cents.

ICHABOD WANTS A PORTFOLIO. Hence a Department of Education. *Ezekiel Cheever's School Issues*, No. 9, June, 1930, 30 pp. *Baltimore* (Warwick and York, Inc.). 30 cents.

JACOBS, M. H., *et al.* Appreciation of Professor Frank R. Lillie on the occasion of his sixtieth birthday. *In* "Anniversary Supplement in Honor of Professor Frank R. Lillie." *The Collecting Net*, June 28, 1930, pp. 1–12.

JOHNSON, ALLEN, and DUMAN MALONE, EDITORS. Dictionary of American Biography. Under the Auspices of the American Council of Learned Societies. Volume V. Cushman-Eberle. *New York* (Charles Scribner's Sons), 1930 Pp. ix + 616. 7 × 10 inches.

MAURICE, ARTHUR B. How they draw prohibition. *Association Against the Prohibition Amendment*, 21 East 40th St., New York, 1930.

MCMURRICH, J. PLAYFAIR. Leonardo da Vinci the Anatomist (1452–1519). *Baltimore* (The Williams and Wilkins Co.), 1930. Pp. xx + 265. 6⅞ × 9⅞ inches. $6.00.

MEAD, MARGARET. Growing Up in New Guinea. A Comparative Study of Primitive Education. *New York* (William Morrow and Co.), 1930. Pp. x + 372. 5¼ × 8 inches. $3.50.

MERRIAM, C. HART. The New River Indians Tlo-Hom-Tah'-Hoi. *American Anthropologist*, Vol. 32, No. 2, pp. 280–293. 1930.

OSBORN, HENRY F. Fifty-two Years of Research, Observation and Publication 1877–1929. *New York* (Charles Scribner's Sons), 1930. Pp. 160. 5¾ × 8⅞ inches. $1.50.

OSTERHOUT, W. J. V. Biographical Memoir of Jacques Loeb 1859–1924. Biographical Memoirs Volume XIII. Fourth Memoir. *National Academy of Sciences*, Washington, D. C., 1930.

PIGHINI, GIACOMO. Viaggi ed Escursioni Scientifiche di Lazzaro Spallanzani. *Bologna* (L. Cappelli), 1930. Pp. xvii + 441. 7¼ × 9¾ inches. 40 lire (paper).

PLATE, L. Review of "Schickedanz, Sozialparasitismus im Völkerleben. Lotus-Verlag, Leipzig (Theodor Thomas Kommissionsgeschäft). Ohne Jahreszahl. 342 S." *Archiv für Rassen- und Gesellschaftsbiologie*, Band 21, Heft 4, 1929. 1 page.

RAPPLEYE, W. C. Current problems of medical education. *Journal of the American Medical Association*, Vol. 94, pp. 915–917, 1930.

ROWELL, CHESTER H. The Kyoto conference of the Institute of Pacific Relations. *International Conciliation*, No. 260, May, 1930, pp. 232–280. 5 cents. (Carnegie Endowment for International Peace, 44 Portland St., Worcester, Mass.)

SHELDON, CHARLES. The Wilderness of Denali. Explorations of a Hunter-Naturalist in Northern Alaska. *New York* (Charles Scribner's Sons), 1930. Pp. xxv + 412. 6⅛ × 9⅛ inches. $6.00.

SMITH, JOHN J. Social Psychology. The Psychology of Attraction and Repulsion. *Boston* (Richard G. Badger), 1930. Pp. xxv + 468. 5 × 7⅜ inches. $2.00.

STIMSON, HENRY L. London Naval Treaty. Radio address. Publications of the Department of State No. 79. *Washington* (U. S. Government Printing Office), 1930. 13 pp.

TROLAND, LEONARD T. The Principles of Psychology. A Survey of Modern Scientific Psychology. *New York* (D. Van Nostrand Co.), 1930. Pp. xxi + 397. 5¼ × 8½ inches. $4.00.

TYDINGS, MILLARD E. Before and After Prohibition. *New York* (The Macmillan Co.), 1930. Pp. ix + 131. 4⅞ × 7⅜ inches. $2.00.

UNITED STATES DEPARTMENT OF LABOR. CHILDREN'S BUREAU. Are you training your child to be happy? Lesson material in child management. *Children's Bureau Publication* No. 202. v + 57 pp. 1930. For sale by the Superintendent of Documents, Washington, D. C. Price 10 cents.

UNITED STATES DEPARTMENT OF LABOR, CHILDREN'S BUREAU. Juvenile-Court Statistics 1928. Based on Information Supplied by 65 Courts. Second Annual Report. Bureau Publication No. 200. *Washington* (Government Printing Office), 1930. Pp. 76. 5¾ × 9⅛ inches. 10 cents (paper).

WHEELER, RAYMOND H. Readings in Psychology. With special readings by Harry Helson, Milton Metfessel and Thomas D. Cutsforth. *New York* (Thomas Y. Crowell Co.), 1930. Pp. x + 597. 5½ × 8½ inches. $3.75.

WHEELER, W. M. The Bussey Institution 1871–1929. Reprint from *The Development of Harvard University since the Inauguration of President Eliot*, 1869–1929, Edited by Samuel E. Morison. Chapter XXXI, pp. 508–517, 1930.

WHEELER, W. M. Review of Auguste Forel's "The Social World of the Ants Compared with That of Man." *Journal of Social Psychology*, 1, pp. 170–177, 1930.

YOUNG, KIMBALL. Social Psychology. An Analysis of Social Behavior. *New York* (F. S. Crofts and Co.), 1930. Pp. xxxviii + 674. 6 × 9½ inches. $4.00.

IV. SOMATOLOGY AND CONSTITUTION

BOYD, EDITH. The experimental error inherent in measuring the growing human body. *American Journal of Physical Anthropology*, Vol. 13, pp. 389–432, 1929.

BOYD, EDITH, and RICHARD E. SCAMMON. The relation of surface area to body weight in postnatal life. *Proceedings of the Society for Experimental Biology and Medicine*, Vol. XXVII, pp. 449–453, 1930.

BOYD, EDITH, RICHARD E. SCAMMON and DONOVAN LAWRENCE. The determination of surface area of living children. *Proceedings of the Society for Experimental Biology and Medicine*, Vol. XXVII, pp. 445–449, 1930.

GOODRICH, EDWIN S. Studies on the Structure and Development of Vertebrates. *New York* (The Macmillan Co.), 1930. Pp. xxx + 837. $5\frac{3}{8} \times 8\frac{1}{2}$ inches. $10.00.

GREGORY, WILLIAM K. The palaeomorphology of the human head. Ten structural stages from fish to man. Part II. The skull in norma basalis. *Quarterly Review of Biology*, Vol. IV, pp. 233–247, 1929.

HARRIS, J. A., C. M. JACKSON, D. G. PATERSON, R. E. SCAMMON. The Measurement of Man. *Minneapolis* (University of Minnesota Press), 1930. Pp. vii + 215. $6 \times 9\frac{1}{8}$ inches. $2.50.

HYMES, CHARLES. The postnatal growth of the cornea and palpebral fissure and the projection of the eyeball in early life. *Journal of Comparative Neurology*, Vol. 48, pp. 415–440, 1929.

KLEIN, ALBERT D., and RICHARD E. SCAMMON. The regional growth in surface area of the human body in prenatal life. *Proceedings of the Society for Experimental Biology and Medicine*, Vol. XXVII, pp. 463–466, 1930.

LINDERS, F. J. Contributions to the knowledge of the stature and its variation within different social strata in Sweden. *Geografiska Annaler*, H. 1, pp. 56–71, 1930.

NAÑAGAS, JUAN C. A comparison of the growth of the body dimensions of anencephalic human fetuses with normal fetal growth as determined by graphic analysis and empirical formulae. *American Journal of Anatomy*, Vol. 35, pp. 455–494, 1925.

NYLIN, GUSTAV. Periodical variations in growth, standard metabolism and oxygen capacity of the blood in children. *Pharmacological Department of the Caroline Institute, Stockholm.* 207 pp. 1929.

REIMANN, STANLEY P. Use and reasons for the use of thiocresol to stimulate wound healing. *Journal of the American Medical Association*, Vol. 94, pp. 1369–1371, 1930.

ST. HILL, KATHERINE. Hands and Faces. The Book of Temperaments. Being the Third and Concluding Volume of "The Book of the Hand." *London* (Rider and Co.), 1930. Pp. 160. 6×9 inches. 10 shillings 6 pence net

SCAMMON, RICHARD E., and ALBERT D. KLEIN. Surface area and age in prenatal life. *Proceedings of the Society for Experimental Biology and Medicine*, Vol. XXVII, pp. 461–463, 1930.

VON VERSCHUER, OTMAR. Demonstrationen zur Zwillingsanthropologie. *Verhandlungen der Gesellschaft für Physische Anthropologie*, pp. 67–74. 1930.

V. PHYSIOLOGY

BINET, LÉON. La Rate. Organe Réservoir. *Paris* (Masson et Cie) 1930. Pp. 117. 6⅛ × 9¼ inches. 20 francs (paper).

BODANSKY, MEYER. Introduction to Physiological Chemistry. *New York* (John Wiley and Sons, Inc.), 1930. Pp. ix + 542. 5¾ × 9 inches. $4.00

CAMERON, A. T. A note on the numbers of molecules and ions present in a single cell. *Royal Society of Canada*, Section V, pp. 151–154, 1929.

CAMERON, A. T. Some notes on our present knowledge and ignorance of thyroid function. *Canadian Medical Association Journal*, Vol. XXII, pp. 240–246, 1930.

CROW, W. B. Voice and the Vocal Apparatus. *Cambridge* (W. Heffer and Sons, Ltd.), 1930. 29 pp. 1 shilling.

FURFEY, PAUL H. The Growing Boy. Case Studies of Developmental Age. *New York* (The Macmillan Co.), 1930. Pp. ix + 192. 5 × 7½ inches. $2.00.

HARVEY, RODNEY B. Plant Physiological Chemistry. *New York* (The Century Co.), 1930. Pp. xix + 413. 5¾ × 8¾ inches. $6.00.

HICKMAN, CLEVELAND P. Laboratory Manual in College Physiology. *New York* (The Macmillan Co.), 1930. xiv + 116 pages. 5 × 7½ inches. $1.10.

HOLMES, ARTHUR D. Cod liver oil and the vitamin theory. *Oil and Fat Industries*, Vol. VII, No. 4, pp. 141–3, 145–6, 1930.

HOLMES, ARTHUR D., and MADELEINE G. PIGOTT. The relative antirachitic activity of cod liver oil and irradiated ergosterol (viosterol). *New England Journal of Medicine*, Vol. 203, No. 5, pp. 220–225, 1930.

HOLMES, ARTHUR D., MADELAINE G. PIGOTT, WM. ALFRED SAWYER and LAURA COMSTOCK. The economic value of cod liver oil and milk as dietary supplements for underweight adults. *New England Journal of Medicine*, Vol. 202, pp. 225–229, 1930.

KOPPANYI, THEODORE. The Conquest of Life. *New York* (D. Appleton and Co.), 1930. Pp. xii + 263. 5 × 7¾ inches. $2.00.

LABAT, GASTON. The trend of subarachnoid block. *Surgical Clinics of North America*, Vol. 10, No. 3, 11 pp., 1930.

LAROCHE, GUY, CHARLES RICHET, FILS and FRANÇOIS SAINT-GIRONS. Foreword by Charles Richet. Translated by Mildred P. Rowe and Albert H. Rowe. Alimentary Anaphylaxis. (Gastro-intestinal Food Allergy.) *Berkeley* (University of California Press), 1930. Pp. 139. 5 × 7 inches. $2.00 postpaid.

MAJOR, RALPH H. Observations on the effects of certain guanidine compounds upon the blood-pressure. *Transactions of the Association of American Physicians*, Vol. XLIV, p. 332, 1929.

MAJOR, RALPH H., and C. J. WEBER. Observations on the depressor substances in certain tissue extracts. *Journal of Pharmacology and Experimental Therapeutics*, Vol. XXXVII, pp. 367–378, 1929.

MAJOR, RALPH H., and C. J. WEBER. The relationship of potassium to the depressor effect of liver extract. *Journal of Laboratory and Clinical Medicine*, Vol. XV, p. 229, 1929.

METZGER, HÉLÈNE. Newton, Stahl, Boerhaave et la Doctrine Chimique. *Paris* (Félix Alcan), 1930. Pp. 332. 5½ × 9 inches. 40 francs (paper).

PAGET, SIR RICHARD. Human Speech. Some Observations, Experiments, and Conclusions as to the Nature, Origin, Purpose and Possible Improvement of Human Speech. *New York* (Harcourt, Brace and Co.), 1930. Pp. xiv + 360. 5½ × 8½ inches. $6.00.

PLATE, LUDWIG. Le vitalisme et le mécanisme d'après une nouvelle conception biologique. *Scientia*, Vol. XLIV, pp. 1–8, 1929.

REBOUX, PAUL. New French Cooking. 300 New and Unique Recipes. Translated from the French by Elizabeth Lucas. *New York* (Alfred A. Knopf, Inc.), 1930. Pp. xiii + 263. 5 × 7½ inches. $2.50.

RIGNANO, EUGENIO. Ni vitalisme ni mécanisme. Réponse au Prof. Plate. *Scientia*, Vol. XLVI, pp. 9–20, 1929.

SNYDER, CHARLES D. On the true anoxidative and oxidative heat production of smooth muscle. *American Journal of Physiology*, Vol. 92, pp. 117–126, 1930.

STOPFORD, JOHN S. B. Sensation and the Sensory Pathway. *New York* (Longmans, Green and Co.), 1930. Pp. xii + 148. 5½ × 8½ inches. $3.00.

SWANSON, EDWARD E., and ROBERT K. WEBSTER. II. The action of ephedrine, pseudoephedrine and epinephrine on the bronchiolar muscle of the isolated lung. *Journal of Pharmacology and Experimental Therapeutics*, Vol. XXXVIII, pp. 327–342, 1930.

VOISIN, GASTON. French Cooking for All. *New York* (Frederick A. Stokes Co.), 1930. Pp. 187. 5 × 7⅜ inches. $2.00.

VI. PATHOLOGY

ALVAREZ, WALTER C. Nervous Indigestion. *New York* (Paul B. Hoeber, Inc.), 1930. Pp. xvi + 297. 5½ × 8⅛ inches. $3.75.

CHITTENDEN, ARTHUR S. Future medicine. 1929. 10 pp. (No place of publication given.)

FRASER, ROY H., and A. T. CAMERON. The use of vitamins A and D and sodium iodide in the pre-operative treatment of Graves' disease. *The Canadian Medical Association Journal*, Vol. XXI, pp. 153–155, 1929.

GLOVER, T. J. The bacteriology of cancer. *Canada Lancet and Practitioner, Toronto*, Vol. LXXIV, 20 pp., 1930.

GOLDSCHMIDT, RICHARD, and ALBERT FISCHER. Chromosomenstudien an Carcinomzellen in vitro. *Zeitschrift für Krebsforschung*, 30 Band, 3 Heft, pp. 281–285, 1929.

MAJOR, RALPH H. Chronic nephrosis: Is it a clinical entity? *West Virginia Medical Journal*, Vol. XXV, No. 8, 14 pp., 1929.

PHYSICIANS HOSPITAL OF PLATTSBURGH. Medical and Surgical Year-Book Comprising Wednesday Afternoon Invitation Lectures, Papers of the Cardiac Round Table, The First Beaumont Lectures, Collected Papers by the Staff. *Plattsburgh, N. Y.* (Superintendent, Physicians Hospital of Plattsburgh), 1930. Pp. xv + 322. 6 × 9¼ inches. $3.50.

SWEENEY, J. SHIRLEY. Co-existent diabetes mellitus and diabetes insipidus with case report. *Endocrinology*, Vol. 13, pp. 477–483, 1929.

THOMAS, STANLEY. Bacteriology. A Text Book on Fundamentals. *New York* (McGraw-Hill Book Co., Inc.), 1930. Pp. xv + 301. 5¾ × 9 inches. $3.00.

VII. METHODOLOGY

ARTSCHWAGER, ERNST. Dictionary of Biological Equivalents. German-English *Baltimore* (Williams and Wilkins Co.), 1930. Pp. 239. 6 × 9 inches. $4.50

BERCZELLER, L. Der mathematische und biologische Collectivbegriff. 4 mimeographed pages. Mimeographed by Dr. Fritz Löw, Wien 8, Josefstädterstrasse 35. No date.

BERCZELLER, L. Ueber Reihengesetze. I. Ueber den Zusammenhang zwischen der Zahl der roten Blutkörperchen und dem Haemoglobingehalt des Blutes bei normalen Frauen. II. Kriminalität und Taglohn. IV. Mortalität und Wohnungsverhältnisse (für Wien). 11 mimeographed pages. Mimeographed by Dr. Fritz Löw, Wien 8, Josefstädterstrasse 35. No date.

COMMITTEE ON STATE ARCHAEOLOGICAL SURVEYS. Division of Anthropology and Psychology, National Research Council. Guide leaflet for amateur archaeologists. Reprint and Circular Series of the National Research Council No. 93. *Washington* (National Research Council), 1930 Pp. 11. 6¾ × 9¾ inches. 25 cents (paper).

DODD, E. L. Probability as expressed by asymptotic limits of pencils of sequences. *Bulletin of the American Mathematical Society*, April, 1930, pp. 299–305.

HAZEMANN, R. H. Introduction à l'étude des méthodes statistiques utilizables en hygiène. *Annales d'Hygiène*, 8e année, No. 6, 13 pp. 1930.

SCHULTZ, HENRY. The standard error of a forecast from a curve. *Journal of the American Statistical Association*, Vol. 25, pp. 139–185, 1930.

TAMMES, TINE. The use of symbols for indicating the history of individuals or groups of individuals in genetic investigations. *Genetica*, Vol. XII, pp. 145–150, 1930.

VIII. MISCELLANEOUS

BRIERLEY, WM. B. Science of the year—1929. The biological sciences *Annual Register*, Rothamsted Experimental Station. 1929. 7 pp.

BUCHANAN, ESTELLE D. and ROBERT E. Bacteriology. For Students in General and Household Science. *New York* (The Macmillan Co.), 1930. Pp. xvi + 532. 5 × 7¾ inches. $3.00.

DERCUM, FRANCIS X. Modern science disclosing the mysteries of nature. *Medical Searchlight*, Vol. VI, No. 8, April 15, 1930. 4 pp.

SCOTT, GEORGE G. Laboratory Manual of General Biology *New York* (Thomas Y. Crowell Co.), 1930. Pp. xi + 125. 5½ × 8⅝ inches. $1.00.

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